

DISCUSSIONS ON Child Development

The Second Meeting of the
World Health Organization
Study Group on the
Psychobiological Development
of the Child
London 1954

EDITORS

J. M. TANNER AND BÄRBEL INHELDER

PREFACE BY PROFESSOR G. R. HARGREAVES

DISCUSSIONS ON
CHILD DEVELOPMENT

VOLUME TWO

DISCUSSIONS ON Child Development

A Consideration of the Biological, Psychological, and
Cultural Approaches to the Understanding
of Human Development and Behaviour

EDITORS

J. M. TANNER

M.D., PH.D., D.P.M.

Lecturer, Institute of Child Health, University of London

BÄRBEL INHELDER

Professor of Child Psychology, University of Geneva

VOLUME TWO

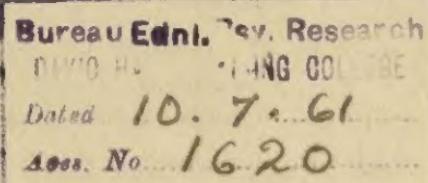
*The Proceedings of the Second Meeting of the
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TAVISTOCK PUBLICATIONS LTD

*First published in 1956
by Tavistock Publications Limited
2 Beaumont Street, London, W.1
and printed in Great Britain
in 10pt. Times Roman by
The Pitman Press, Bath*

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MEMBERS OF STUDY GROUP

DR. JOHN BOWLBY

Director, Children's Department
Tavistock Clinic, London

Psychoanalysis

DR. FRANK FREMONT-SMITH

Chairman

Josiah Macy Jr. Foundation, New York

Research promotion

MLLE. BÄRBEL INHELDER

Professeur de Psychologie de l'Enfant
Institut des Sciences de l'Education de
l'Université de Genève

Psychology

DR. KONRAD Z. LORENZ

Forschungsstelle für
Verhaltensphysiologie des Max-Planck
Institutes für Meeressbiologie
Bulldern über Dulmen, West Germany

Ethology

PROF. G. R. HARGREAVES

Formerly Chief, Mental Health Section
World Health Organization, Geneva
Professor of Psychiatry
University of Leeds

Psychiatry

DR. MARGARET MEAD

Associate Director Dept. of
Anthropology
American Museum of Natural History
New York

Cultural Anthropology

DR. K. A. MELIN

Director, Clinic for Convulsive Disorders,
Stora Sköndal, Stockholm

Electrophysiology

DR. MARCEL MONNIER

Chargé de Cours de Neurophysiologie
appliquée, Université de Genève

Electrophysiology

PROFESSOR JEAN PIAGET

Professeur de Psychologie à la Sorbonne
et à l'Université de Genève

Psychology

DR. A. RÉMOND

Chargé de Recherches, Centre National
de la Recherche Scientifique, Paris

Electrophysiology

DR. R. R. STRUTHERS

Formerly Associate Director
Rockefeller Foundation, Paris

Research promotion

DR. J. M. TANNER

Formerly Senior Lecturer, Sherrington
School of Physiology, St. Thomas's
Hospital
Lecturer, Institute of Child Health
University of London

Human biology

DR. W. GREY WALTER

Director of Research
Burden Neurological Institute, Bristol

Electrophysiology

RENÉ ZAZZO

Directeur du Laboratoire de
Psychobiologie de l'Enfant
Institut des Hautes Etudes, Paris

Psychology

GUESTS

DR. DALBIR BINDRA

Department of Psychology
McGill University
Montreal, Canada

DR. D. BUCKLE

Regional Officer for Mental Health
Regional Office for Europe
World Health Organization
Geneva

PROF. HOWARD LIDDELL

Professor of Psychobiology
Cornell University
Ithaca, U.S.A.

PROF. JOHN W. M. WHITING

Laboratory of Human Development
Harvard University Graduate School of
Education, Cambridge, U.S.A.

PREFACE

Readers of the first volume of this series will know that at its first meeting the W.H.O. Study Group on the Psychobiological Development of the Child based its discussions on a series of presentations each of which covered the general views on child development of one of the various disciplines represented in the membership of the group.

At its second meeting, held in 1954, the group based its discussions on presentations related to a broad common theme—Learning, with special reference to learning under stress and to learning in the immature organism.

The meeting lasted for six working days. Half a day was devoted to the discussion of each presentation.

The topics presented included electro-mechanical models of aspects of learning (Grey Walter), the effects of perceptual deprivation in man and animals (Bindra), learning under stress in animals (Liddell), and the cross-cultural study of learning of internalized standards of behaviour (Whiting).

In addition two films provided material for discussion, the first on motor behaviour patterns in the premature human infant (Lorenz) and the second on the response of a two-year-old child to hospitalization (Bowlby). Other subjects that entered into the discussions included an account of the study of the development of abstraction in learning, by Professor Inhelder, and the measurement of opto-motor-cortical time as a parameter for studying learning, by Dr. Monnier.

The enthusiasm of the group and the ability of the Chairman, Dr. Frank Fremont-Smith, led to a week of far-reaching and vigorous discussion.

Dr. Tanner and Professor Inhelder, the editors of this series, have again undertaken the task of condensing to the dimensions of a single volume the lengthy verbatim transcript of the meeting and have carried it out with a skill that even the victims of their blue pencils admire.

G. R. HARGREAVES

Lately Chief, Mental Health Section
World Health Organization

Leeds University

CONTENTS

PREFACE	<i>page</i> 9
INTRODUCTION	15
1 Presentation: Dr. Grey Walter	21
2 Presentation: Dr. Bindra	75
3 Presentation: Dr. Liddell	123
4 Presentation: Dr. Whiting	185
5 Presentation of a Film by Dr. Bowlby	213
6 Presentation of a Film by Dr. Lorenz	235
INDEX	269

PLATES

FIGURES 1, 2, 8A, 11	<i>facing page</i> 32
FIGURES 12, 13	33
FIGURE 14	64
FIGURES 15, 19	65

Introduction

FREMONT-SMITH (Chairman):

Mesdames, Messieurs, may I call this second session to order. We are exceedingly happy that we have our three guests present with us for this meeting. We will not all go through our autobiographies as we did last time (see Vol. 1). But we do want to hear from our guests something as to their background and the interests that led them to attend this Study Group on the Psychobiological Development of the Child.

First let me remind them and all of us as to what we aim at for the mood and technique of these conferences. We are a multi-professional group, each of us trying to stretch our outlook to include some approach to or some aspect of science of which we were ignorant or only dimly aware before. We are trying to *communicate* with each other, not, as in the ordinary scientific meeting, to *make statements at* each other. The two are very different processes.

New ideas, that is, the new findings of others, are likely to produce a certain amount of anxiety, especially if they challenge our area of interest and competence. This is something which it is hard for us to accept—the idea that another scientist would make me feel anxious is rather intolerable—so what one does quite naturally is to suppress this anxiety, and as you know, anxiety suppressed does not disappear, but is transformed very quickly—into hostility. One can take it as a rule of thumb that new ideas from others, challenging our area of interest, almost spontaneously evoke hostility in us. This is why there is such difficulty in communication among scientists, and particularly across the disciplines.

The easiest reply to make to such a hostile idea, or hostility-provoking idea, is to say 'That's nonsense. That's not so, that is not in accordance with the facts'. But with a good friend the position one takes is different: by definition he is somebody who makes sense and whom one wants to communicate with. So instead of saying 'That's nonsense' we would say 'But I could not quite have understood you; I did not have the insight, explain yourself further'. In a group like this what we aim at generating is the diametrical opposite of the psychoanalyst's free-floating anxiety. We need

free-floating security, so that instead of having multiple swords of Damocles hovering above the group waiting to plunge we have an atmosphere in which we can say what we like when we like, and in which the ideas of others provoke interest and not hostility.

I will now ask our three guests to introduce themselves.

WHITING:

I think perhaps I should start with my education. In my undergraduate days, except for an introductory freshman's course in biology, I took nothing which anybody might possibly talk about here. I did the easiest courses in history and English literature that I could find. I was an expert poker player, however, and I was captain of the wrestling team.

As a graduate student I started out in the field of sociology at Yale and after the first year I switched over to anthropology. At this time, Edward Sapir and John Dollard were associated with the Department of Anthropology. I got interested in the problem of personality development, or culture and personality, and I read *Coming of Age in Samoa* by DR. MEAD (1928) which had a great influence on me. I became interested not only in anthropology but also in psychology, particularly psychoanalysis, and started to get training in analysis and to carry on work in that field. For my anthropological field work, I went to New Guinea, in Margaret Mead's general territory, and did a study of child training in a group up the Sepik River.

Up to this point I had hardly heard of learning theory. It seems rather strange, therefore, that I am here, presumably, to represent learning theory. I became interested in it about a year after I got my doctor's degree and worked with Hull, primarily at the Institute of Human Relations, and Miller, Sears, Marquis and Mowrer, who were all there at the time.

During this post-doctorate training, I worked on a learning theory problem which was an experimental one. I also tried to apply what I had learnt in New Guinea on child training and child development. That is, I reinterpreted my field notes and the result of this reinterpretation finally came out in a book (WHITING, 1941).

Since that time, I have worked trying to put together cultural anthropology, psychoanalysis and learning theory into a more or less coherent framework so as to understand the development of the child, that is how the child becomes an adult member of society.

After the war, I went to the Child Welfare Research Station of the State University of Iowa, where I started some research with Bob Sears. After a couple of years there, I returned to Harvard with

Sears and set up a laboratory with him of which I am now the Director. We continued our work in child development both with data on children in the setting in and around Harvard and from published literature on children around the world. We also sent people into the field to observe children directly.

I think I should end by begging a complete ignorance of biology, but at the other end of the spectrum, I may be joining with many of you here at the psycho- part of psychobiology.

LIDDELL:

I think all of us who continue in the experimental investigation of behaviour become of necessity evolution-minded and history-minded. It is certainly true in my own case.

First a word about the history. My own investigations derive very definitely from the British Isles. At the turn of the century Professor E. A. Schaefer, who later became Sir Edward Sharpey-Schaefer, was doing a classical investigation with his colleague, Oliver, at Edinburgh. It was a study of the effects of injecting adrenal extract into a mammalian preparation specifically to observe the effect on blood pressure. At that time, Professor Schaefer had already changed his interest from neurophysiology and neuroanatomy to the endocrine glands. He was invited to give the Lane Lectures at the University of California Medical School in San Francisco, out of which came his book, *The Endocrine Organs*. He stopped to lecture at the Ithaca Division of the Cornell Medical School, and this School was in search of a professor of physiology. Schaefer recommended Sutherland Simpson, who was his lecturer. Simpson became Professor of Physiology in our medical school in 1909, and I joined him as instructor in 1919.

My own work over these many years with sheep and goats depends upon two modest physiological facts. Firstly, in sheep and goats the inferior parathyroid glands are free in the neck and not embedded in the thyroid, and therefore the removal of the thyroid gland from a lamb or kid three weeks of age is a simple operation under local anaesthetic and parathyroid tetany does not supervene. Secondly, twinning is a frequent occurrence in both sheep and goats. Dr. Simpson put these two facts together. He wished to engage in natural history or case-history physiology—the chronic experiment. So from 1909 until his untimely death in 1925, he was devoted to his new field of endocrinology with special reference to the thyroid, trying through the chronic experiment on the thyroidectomized animal to contribute to clinical endocrinology.

My own academic history is very simple. I entered the University

of Michigan, began preparation for medicine, then changed to psychology. My psychology teacher, Professor Pillsbury, had been a pupil of Edward Bradford Tichener of Cornell. I came to Cornell in 1918 as a graduate student of Professor Tichener and received the conventional instruction in structural psychology and learned how to introspect in structural terms. But Tichener conferred on me a great benefit. He saw to it that I took as a minor subject medical physiology. There I encountered Professor Simpson and in due course became his research assistant. His experiments were simple in design and long-continued in operation. He selected twins of the same sex, kids or lambs, thyroidectomized one of the pair at three weeks of age and observed both until the death of the operated animal. Since he could demonstrate most of the signs of athyroidism, he said, 'Since you are a psychologist, it would be interesting to demonstrate the blunted mentality in these thyroidectomized sheep and goats'. But what about the mentality of the sheep and goat? Has it any mentality to be blunted?

I was committed to becoming a psychologist again, because Simpson made me responsible for the animals' behaviour observed day by day in the pasture and the laboratory. Having to teach physiology, I read Pavlov's work on the digestive glands and his Huxley Lecture in the *Lancet* and resolved to apply a really physiological method to this field and be done with the nonsense of psychologizing the sheep and goat. So I began to demonstrate defects in conditioned reflexes in these sluggish thyroidectomized animals, and each time the animal came to the laboratory it became a laboratory preparation and I bid it good-bye at the door; my responsibility for it was done. But it was not that simple. It became necessary to put pedometer watches on the animals to get diurnal activity in the pasture. It also became necessary to study their diurnal fluctuation of body-temperature. Then, insensibly, I came back into what I think is fairly called the field of psychology, and overcame my squeamishness about anthropomorphizing. I think one can say that in studying an animal, homology may throw light on behaviour as it does on structure. The sheep is not a man; nevertheless, we can freely empathize; we have our own view as to how we would regard the situation if we were in the animals' place; and we have taken this matter seriously. Every single procedure which we apply to our animals in the Pavlovian situation we try on ourselves, and this has led to important clues.

It might interest some of you to know my own contact with the word psychobiology. I heard this word first in 1919—I think at the American Psychological Association on a conference on nomenclature. Robert Yerkes insisted on inserting the word 'psychobiology'

as an acceptable psychological term. No-one else paid any attention to it. Yerkes, however, persevered, and he is now Emeritus Professor of Psychobiology at Yale. Then the issue was confused by Professor Adolf Meyer, the Director of the Phipps Clinic at Hopkins who caused his school of psychiatry to be known as psychobiology. Adolf Meyer also supported experimental psychobiological work with animals in his clinic, where W. Horsley Gantt still continues Pavlovian conditioning.

BINDRA :

My undergraduate work was done in India at Punjab University, where I specialized in zoology and psychology. For my graduate work I went to Harvard and worked with Boring and Allport. The subject of my doctoral dissertation was hoarding behaviour of rats. After obtaining my degree in 1947, I went down to teach in a small American university in Washington, D.C. A couple of years later I joined Professor Hebb at McGill and have been there since.

My interests in psychology at the present time revolve around the theory of emotion and motivation. My research makes use of both human and animal subjects. Our general approach to all problems involves developmental (ontogenetic) and phylogenetic comparisons. I must admit that at McGill our idea of phylogenetic comparisons is that of comparisons between rat, cat, dog, chimpanzee and man, and Dr. Lorenz's discussions of sub-mammalian animals makes us feel guilty for our sins of omission.

So much for my research interests. Another side-interest that I have followed for some years is that of scientific method, and I feel that a consideration of the relation between common sense and science is particularly relevant to discussions of many psychological problems. My feeling is that psychological concepts, on the whole, are still more or less common-sense concepts and have not been developed to any degree of precision. These common-sense psychological concepts are undoubtedly very useful in everyday life and are useful in clinical practice; but they are not good enough for scientific purposes. By way of analogy, consider a layman's concept of hot and cold: it is a cold day outside or it is a warm day outside. These judgments of cold and warmth are very useful, they tell you whether or not you need to wear a coat; but any scientific treatment of cold or warmth will have to be in terms of certain dimensions such as temperature, humidity, wind velocity and things of that kind.

Many psychological concepts in the area of emotion and motivation are common sense concepts, such as fear, anger, jealousy, anxiety, and I feel that unless we are able somehow to replace these

concepts by more precise ones, analogous to those of the physicist's temperature, humidity, and so on, we will probably continue to run into difficulties. This is relevant to the difficulty which has been mentioned by Dr. Rémond. It is the difficulty of relating EEG measures to psychological or behavioural concepts. The reason we are unable to relate anxiety, anger, fear, and so forth to electro-physiological measures lies partly in the vagueness of these psychological concepts. It is the psychologist's task to refine psychological concepts before asking for electro-physiological correlates of psychological events.

FIRST DISCUSSION

Presentation: Dr. Grey Walter

GREY WALTER:

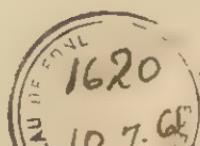
To begin with, I want to put before you the proposition that the psychobiological development of living creatures can occur in six ways:

1. Genetic evolution (mutation, selection, etc.)
2. Reflexive action (tropisms, archisms, taxias)
3. Instinct (innate releasing mechanisms, imprinting, etc.)
4. Practice (learning by repetition)
5. Learning by association (conditioned reflexes)
6. Social communication (insect up to human communities)

I do not say it can occur only in six ways, though I am rather inclined to believe this is true, and that there is no other way possible. I should like to include with 'living creatures' perhaps also 'artificial creatures', such as we shall discuss in a moment.

The first proposition which I am maintaining is that these six methods of change or development are the largest single categories we can recognize for convenience in study, though in fact we know that these six categories mingle in an extremely elaborate way. The division into six is based not on an arbitrary criterion but on the empirical one of experiment. One can detect differences between the mechanisms and recognize each by quite diagnostic traits. One of the virtues of this systematization is that it helps one to list and understand one by one the important factors in development which depend upon a nervous system.

In order to decide what the nervous system *must* be able to do apart from what it *might* be able to do, it is useful to recall the capacity of plants. Of the six methods of development that I have listed, I suggest that plants possess only the first and second, and perhaps in some cases the third. The first method of development



is that of *genetic evolution*. Mutation and selection are included here, and all methods which involve change of character or behaviour from individual to individual, whether or not this necessarily results in the development of a new species. Evolution of this type does not depend essentially on the nervous system.

The second category of development I have called *reflexive action*. This use of the word 'reflexive' is my own choice and in it I include both the artificial reflexes of the laboratory of classical Sherringtonian type and the tropistic, archistic and taxistic responses such as occur for example in infusoria, in insects, and also in plants. There are many examples of plants which devour and digest a variety of foodstuffs, which orientate themselves to the sun, adapt themselves to a support up which they climb and so forth. A nervous system is not essential for reflexive action but its possession does permit a much wider variety of reflexive modes than we find in plants.

In the third category labelled *instinct*, I include all the types of behaviour which Lorenz and his ethological colleagues have made, if not familiar, at least very attractive to us. I feel more and more inclined to study this type of behaviour and its physiological concomitants and I think we shall soon see the way in which this can be done. Instinct can at least be defined clearly and the sort of nervous system that is necessary and perhaps sufficient for behaviour of this type can also be to some extent defined.

The fourth heading is *practice (learning by repetition)*. This is difficult to define in detail, but perhaps I can dispose of it now by saying that change of behaviour by repetition is at once so trivial and so profound that it has almost nothing to do with the unique properties of animal behaviour. The improvement in performance which occurs when any mechanism whatever continues to work is universal in occurrence. As examples, I suggest the running-in of machinery, the erosion of a river bed by a river, the change in a billiard table after it has been played on, the change of the shape of shoes after wearing—the change induced in any one system by contact and coupling with another. In the true animal systems this type of change by repetition, which we call practice, has special characteristics and I certainly do not mean to deprecate the importance and beauty of it. But it is not a specifically organic or animal property. This is sometimes forgotten. The improvement of performance as rated in the laboratory situation is often the same as improvement of performance in any mechanism turned on for the first time and allowed to run in, and to regard this as having a special property and a special interest may, in certain circumstances, rather confuse the issue and make experiments harder to understand and more difficult also to repeat.

BINDRA:

In this item, learning by repetition, do you include such things as fatigue?

GREY WALTER:

No, not necessarily. In some particular case there might be evidence that the effect was a running-down rather than running-in, but these are not the same.

BINDRA:

Fatigue does refer to behavioural modification resulting from repetition. But I see that if you did include fatigue under 'change in performance with repetition' it would complicate your scheme.

GREY WALTER:

I do not include fatigue unless there is some special reason; for example unless fatigue was the result of practice, when it could be included as a running-in process. But 'fatigue' designating stocks of metabolites exhausted, say, or the failure of a membrane potential, that is not learning by repetition. That is a much more complex process.

BINDRA:

You are restricting your category to *learning* by repetition?

GREY WALTER:

It has to be a change in behaviour which occurs by repetition of an act and which also is *adaptive*, that is permits tighter coupling between organism and environment.

BINDRA:

But, then, does the shoe that fits better after use fall into this category?

GREY WALTER:

Yes. Your foot may 'learn' to fit the shoe. That would be the same process.

BINDRA:

That is learning?

GREY WALTER:

No. I would not call a process which involves merely practice 'learning'. Learning is a rather precious word and I should restrict it to the other types such as I am describing. ECCLES (1953) has shown that some degree of practice effect can be detected even in the spinal cord reflex system. He isolated a reflex preparation for a long period and then applied a stimulus and found improvement by repetition of the stimulus. He does regard this as learning. That practice can occur at the spinal level is an important observation, but I do not regard it as any more significant than the fact that if you allow a machine to rust and then use it again, it will be some time before it gets up to its maximum speed.

The fifth category I define as *learning by association*, of which the main example in the scientific field is generally called a conditioned reflex. A large part of my contribution will be about this. It introduces notions which I find hard to understand because I am extremely inept at devising or comprehending mathematical notations, and the algebra involved in the mathematical aspect of their study is formidably complex. For me it is easier to consider these problems in the form of a mechanical embodiment or model of the equations.

PIAGET:

May I interrupt? To make clear the difference between (4) and (5), would you agree that (4) is what one usually calls 'exercise' in French and (5) 'acquired experience'?

GREY WALTER:

No. I should say my discrimination between the two would depend upon number five including adaptation to two series of events, not one. Learning by practice, or the running-in of machines, depends simply on the repetition of a certain action which wears down resistances, obstructions and so forth and makes for smoother running. Whereas number five, as I shall show in some detail, depends upon the adaptation to a combined contingency of two series of events.

The sixth category, which again is defined too crudely to bear repetition outside this circle, is *social communication*—the development of creatures by means of social intercourse. Even in the lowliest animals there is some indication of social development. Insects display it to an intense degree. For social communication to occur it is far from essential to have a very complex central nervous system though whether one can reasonably imagine social plants seems to me doubtful.

These six categories are operational divisions which are based upon the possibilities of recognizing the differences between them. In any real case, however, all these factors are entangled, and it is very difficult to design an experiment in which any one of them is reasonably faithfully magnified without distorting the others, or in which all the others are removed without distorting the single factor that you want to study. Most of us here have attempted at one time or another in our experimental lives to isolate a single factor in the classical tradition of the experimental physiologist. Such is classical physiology, classical chemistry, and classical physics, but never classical psychobiology. That is our difficulty. It is no good saying; try to isolate a single variable. In isolating it we are destroying the system we are studying, and efforts to record just one factor very carefully and very accurately over a very long time are of academic interest only, and tell us more about the experimenter than about the subject.

In the application of electrophysiological methods one stumbles at the threshold over an extremely awkward barrier, that of technical description—how to display to untutored eyes changes in time and space of a dozen or two dozen variables. For us who work in electrophysiology to convey to you, not merely the nature of our studies, but also their range and reliability means that we would have to introduce you to some of the extremely tedious technical tricks which we use. We would have to confess to you the degree of our inaccuracy; to take you with us to our laboratories and say: 'This we can measure, that—not yet'. We should have to inflict upon you a shortened intense course in electrical technology. The years since the last war are an unfortunate stage in this work. In ten or twenty years we shall have as much confidence in telling you what we have seen as the astronomer describing the nature of some remote galaxy, or the spectrum of a star. He does not have to instruct us in the principles of the spectroscope or the telescope, he just has to tell us there is a galaxy which is such and such a size moving at such a velocity, and we believe him. Electrophysiologists, however, are still in the embarrassing technical stage, when we wish to participate in meetings of this sort, of having either to sell an idea in a bag, or describe very exactly what the bag is made of. I am going to do neither of these things, but if anyone would like to have more information or if, during the weekend, anyone would like to see for themselves the hardware that churns out these results, they are very welcome to come to visit us in Bristol! If you enjoy hardware we can give you a good time. But there is not the slightest reason why you should come into the ironmongery shop at all.

If one avoids this ironmongery, one comes to an alternative way

of describing the results obtained. From our observations and the hunches we have had in studying human beings in relation to the subject of this conference, we have built up a number of hypotheses, and I am going to asseverate that when one has a notion which purports to explain observed phenomena, one has a right to turn one's hypotheses into a model. The observation of these models has the same degree of validity as the study of animals, if you are relating your studies to human problems. In studying animals one is studying what one supposes to be a model of human behaviour, and one is perfectly at liberty to anthropomorphize—'the animal does so and so, and this is what I feel when I do the same thing'—if it makes thinking clearer and more vivid and makes the hypotheses more precise and conclusive.

The same applies to the study of a model. A model, provided it fulfils certain criteria, can be just as useful, just as helpful, and in some cases almost as charming, as the animal which it replaces. But the scientific laws one has to obey in making models are very strict. The first is the principle of parsimony; that entities must not be multiplied beyond necessity; in making a model, you must not include a single redundant part or component, you must not have a lot of frills that are not necessary. You must take away anything superfluous from the model and see if it still works and if it does then you have to think again. This can be quite a tedious process.

Here I might possibly mention some of the devices quoted as being models of human behaviour, that is, electronic computing machines. We have all heard of, and many of us have seen or used, computers of various types. I think that the tendency to use these electronic devices as models of brain activity and human behaviour is an unfortunate one, because these computers do not observe the principle of parsimony. They are enormous things with a vast quantity of redundant parts, designed particularly to do well what we do badly—addition sums and so forth—and being designed to do exactly the things which we find extremely difficult, they bear no more relation to the human brain than a hacksaw does to the human hand. Their enormous number of superfluous components rules out their interest as models of the sort of things we are talking about.

It may help us to see why the models I am going to show are different from computing machines if I discuss for a short while the nature of various systems of communication. Most computing machines operate on a binary system, the system in which there are only two possible assertions: yes and no, one and zero. A large part of mathematical computation can be handled on this basis, and many questions of logic also.

Another system which has been developed, oddly enough more

recently, has been particularly profitable in the hands of UTTLEY (1954), an engineer-physicist at the Radar Research Establishment in Malvern. This system is a unitary one, it can only say 'yes' and that is all. In other words, nothing does not mean 'no', it means nothing.

There is a third category which one can call the plenary system. This is really much more relevant to our problem, because it resembles more closely the sort of thing that happens in our experience. An example of a plenary system is an ordinary telephone system. If you dial a number, several things can happen. Suppose you are a burglar and you are doing what they call in England 'sounding the drum', that is you are ringing up a house to see if there is anyone at home. You dial, and if the person answers that is all you want to know and you hang up. This is, of course, a 'yes' response; but in a telephone system a number of other things may happen. You may hear the bell ring but get no answer. This tells you nothing at all except that the bell is ringing. You may get a wrong number; you may get complete silence, the number may be engaged, or there may be a fault. That is a plenary system in which 'yes' and five ways of saying 'no' exist. This is much more like our experiments. You feed your animal and the response may appear or all sorts of other things may happen instead. If the animal does not respond you do not know whether it is dead, tired, sleepy, if you have stimulated it properly, if it has already been stimulated, or if it just is not interested.

The models I have brought with me are mostly unitary systems. They are the simplest. One model, however, is a plenary system in which almost anything may happen, apart from the simplest response of 'yes'. This has rather interesting relations to logic and semantics. If you say to somebody, 'Do you understand me?', and they say, 'Yes', that conveys no information at all. If they say, 'No', that probably does. If you say, 'Can you hear me?' and they say, 'No', what information can that convey?

The next stipulation in model-making is that one must know more than one feature of the system one is going to copy. This may sound childish, but it is often forgotten. If one wants to make a model of a bicycle, one must know more than that it has two wheels. A great many things have two wheels, but not all of them are bicycles. But, if one makes a thing which has two wheels and can be ridden, the chances are it will be a bicycle. However, if you make something that can be ridden, it need not have two wheels. An important and often forgotten point about the making of hypotheses or models is that you cannot make a model of something very simple. For example, if one wants to make a model of a seesaw, well, the model of a seesaw is a seesaw.

The third very stringent requirement, of course, is that a model must be found to reproduce more than has been put into it, which is the same thing exactly as saying that a hypothesis must permit prediction. It is no good if your model does something curious which you cannot understand; it must actively develop some mode of behaviour which you had not thought of first and had not deliberately built into it and which enables you to see that the thing it is a model of must show the same unexpected behaviour.

I have not attempted to make models of the first and fourth of the methods of development which I have listed (genetic adaptation and practice adaptation) and for various reasons I do not think they are worth making at the present time.

I will carry on now with the description of the first model I want to show you, which will be available during the rest of the meeting if anybody would like to play with it. This model demonstrates purposive behaviour and the first proposition I am going to put to you is a very general one; that so-called purposive behaviour can be defined in terms of reflexive action without recourse to transcendental teleology. It is an important first principle to establish that the so-called 'purpose' which, until recently, was always regarded as an essential and intrinsic diagnostic attribute of animals does not depend on and is not diagnostic of the presence of an elaborate nervous system as such.

The second proposition I wish to make is this: that the classification by a purposive mechanism of experience as relating to 'self' or 'not-self' is bound to occur when the reflexive circuit includes an environmental operator. (An operator is an indication of how two or more terms influence one another. A multiplication sign is a simple operator which says how two numbers are to be joined. In this particular case, the operator I am referring to tells us how the organism and its environment are to be joined together.) The proposition here is that the responses which are characteristic of the organism noting its own existence are distinguishable from the responses that occur when the organism notes that something outside itself has happened.

The third proposition, which depends on the same mechanisms, is that one gets the impression of social organization; the setting-up of a social complex is bound to occur when two or more reflexive mechanisms of the type previously defined interact one with another. What I am saying is that, if you construct reflexive mechanisms, then these two things, the classification of 'self' and 'not-self', and the formation of a social organization, are bound to occur. In other words, these two processes are predictable from the purposive nature

of the mechanism, and you require no more information than that the mechanism is purposive to foresee the recognition of self and not-self and the formation of a society.

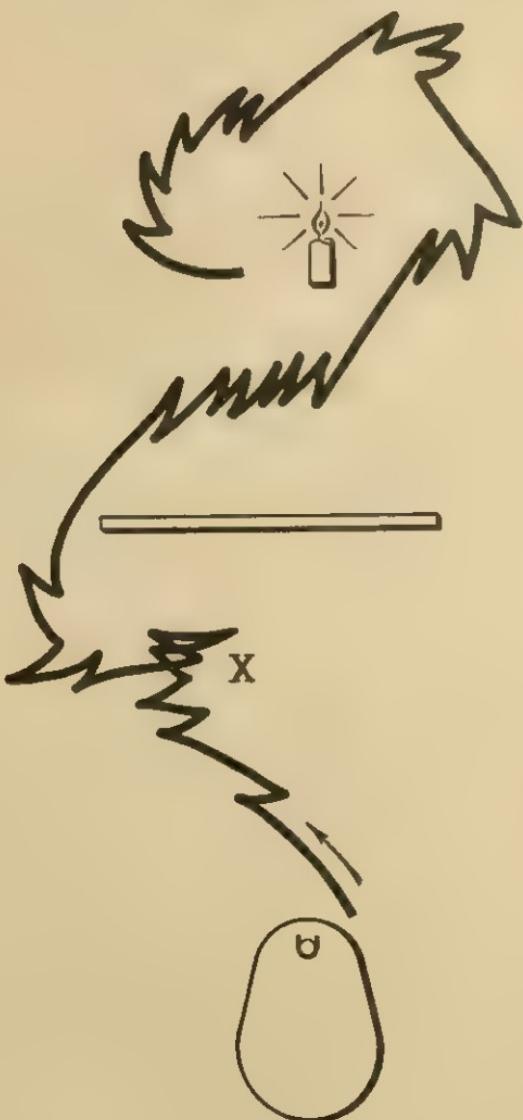
This model is a unitary system and contains two elements. It is like an animal with two nerve cells, each of which is a unitary system. It either says yes, or it says nothing. It has been described in detail elsewhere (WALTER, 1953) but it is as well to remember that in a system with n elements, the possible number of modes of behaviour is $2^{(n^2-n)}$. That means if we had six neurones in our brain, or six nerve elements (which might have many neurones in each), we should have enough elements in our heads to provide us with a new sensation every tenth of a second for the whole of our lives. Such is the richness of inter-connexion. The expression is exact only when n is a large number; it is only approximately true when n is less than 10. As you can see, this is an explosive series; it is one of the interesting consequences of this type of model-making—though I realized it only after I started making these toys—that a very small number of nerve elements would provide for an extremely rich life.

I am now going to show some pictures of one or two of these unitary, two-element creatures. They are designed merely to indicate how complicated the structure of behaviour can be, even when the anatomical structure is simple. The thing to look at in all these pictures is the trace of the line of light which is the track of the creature under various conditions.

The particular creature illustrated in Fig. 1 (facing p. 64) has two receptors, one of which is sensitive to light and the other to touch. In Fig. 2 (p. 64) we see the response to light. The light is the goal that the track leads to; you see that the creature has a tropism, confined to a response to light. The choice of light in this particular model is of some importance because it is very easy to make light represent food; these models 'eat' electrons, and where there are electrons there can be light. They will chase light and where light happens to be associated with a suitable supply of electricity, there they will plug themselves in and charge their batteries should they require to do so. When nourishment is over they will leave the feeding trough and wander about in search of adventure. The specific name for this animal is *Machina speculatrix*—the Spying Machine. They have one unit of curiosity. They are capable of investigation and will explore as long as they retain their power, only desisting when their metabolism is strained beyond bearing.

In Fig. 3A (overleaf, p. 30) we have a slight complication in the situation to which these creatures are subjected. Here the creature, starting at the bottom, catches a glimpse of the distant light, a candle

FIG. 3A



WHO 6140

on the other side of an opaque screen. The creature starts off for a moment in the direction of the light, but is very quickly baffled by the fire-screen which cuts out the light. It then makes a circle round to the left. At the point marked X an accident occurred which

is characteristic of model hypotheses. To make these photographs an ordinary kitchen candle was stuck on the creature's back and at point X the creature caught sight of the reflection of its own candle in the polished fire-screen; so there it stuck, and it took some time for it to make a readaptation and concentrate on its goal. However, it finally circled around the fire-screen and got into an orbit round the attractive light. This indicates the way in which having once got a line on some possible attraction, the creature will take it up again, even after it has lost sight of the original goal. It has no power to remember, no power to adapt in the sense of the later categories I spoke of; it only contains tropistic reflexive mechanisms.

FREMONT-SMITH:

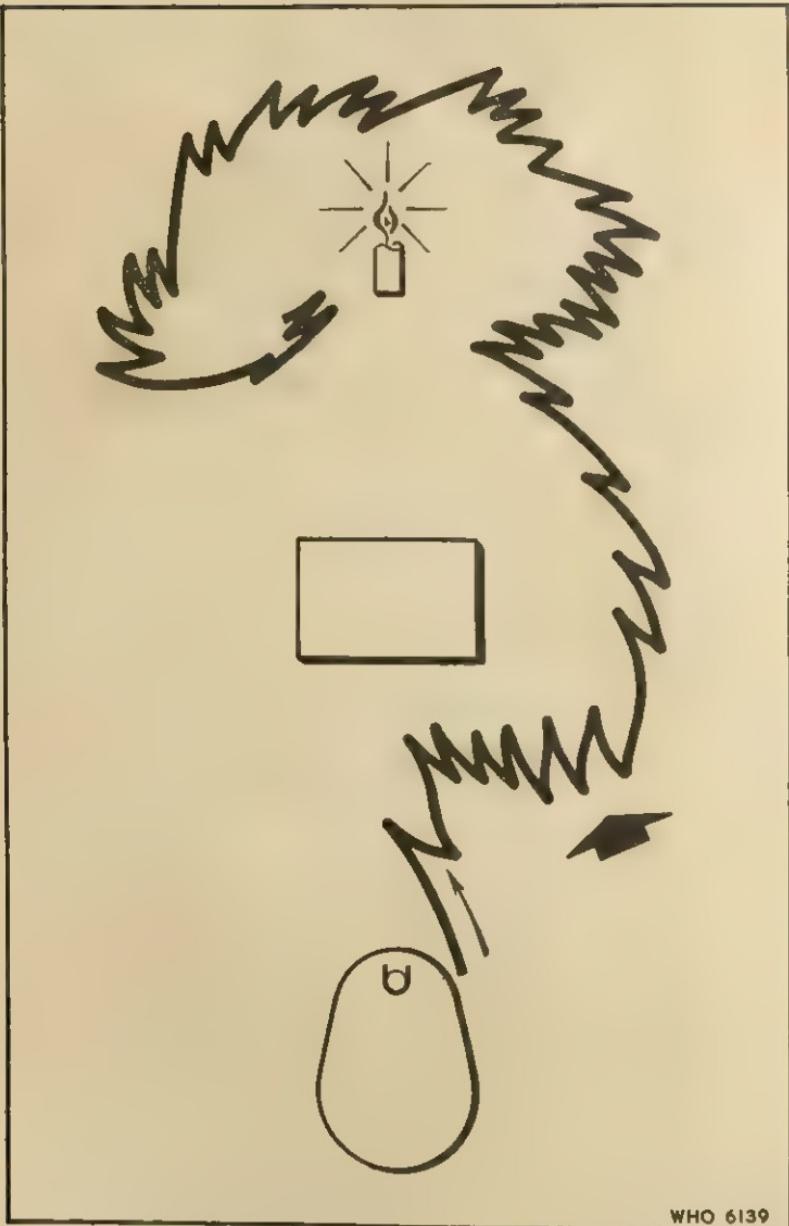
How does it keep after the light?

GREY WALTER:

If you have a scanning device, a rotating photo-cell, then once it gets on to a line it will tend to pick up that line again within a reasonable time. The same unfortunately is true of self-guided missiles. If a missile is aimed at London and interrupted by some counter-force, it is easy to make it take up the line again, after the perturbing force has been circumvented. This is characteristic of quite a simple system without storage.

In Fig. 3B (p. 32) is an arrangement in which the creature starts at the bottom of the picture and is attracted towards the candle again at the top; but in between there is a low obstacle over which it can see the light, but through which it cannot move. It has to circumvent this obstacle and its touch mechanism comes into operation. It starts off straight towards the candle, it touches the box and dodges it by a series of backing and butting movements. At the point marked by an arrow there arises by sheer accident a storage system; if A represents one element and B the other, the feedback mechanism A B A B A, etc., occurs. This has brief storage, so that if the model encounters an obstacle it will not only avoid it, but will go on doing so until it is clear of the obstacle, a consequence of the way the act is done and not of the way the machine is designed. One of the predictions of this hypothesis is that if a reflexive mechanism which is capable of two modalities or sensations is engaged in one modality, it will not, at the same time, be able to cope with the other. In other words, in this particular case it is 'more important' for the creature to circumvent a material obstacle than to chase a distant light, and the circumventing of the obstacle immediately cuts out the photo-electric responsiveness.

FIG. 3B



WHO 6139

The remaining pictures show situations of increasing complexity and difficulty for the model. Fig. 4, I think, brings the thing a little bit nearer to the field of biology, because here we have evidence that a system with two elements of a reflexive nature may have a tendency

FIG. 1

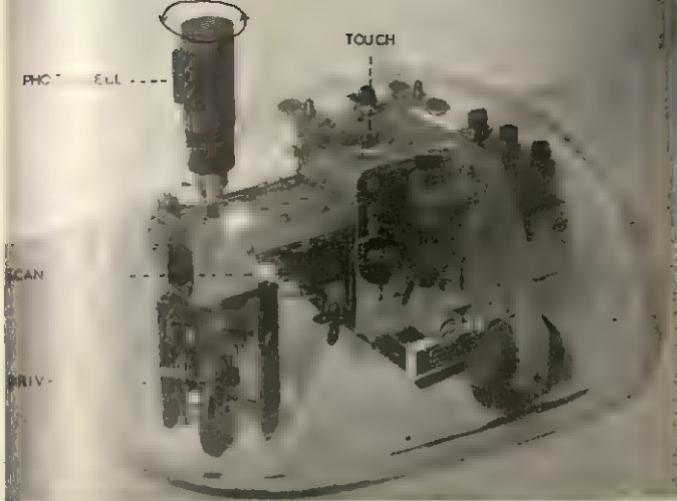


FIG. 2

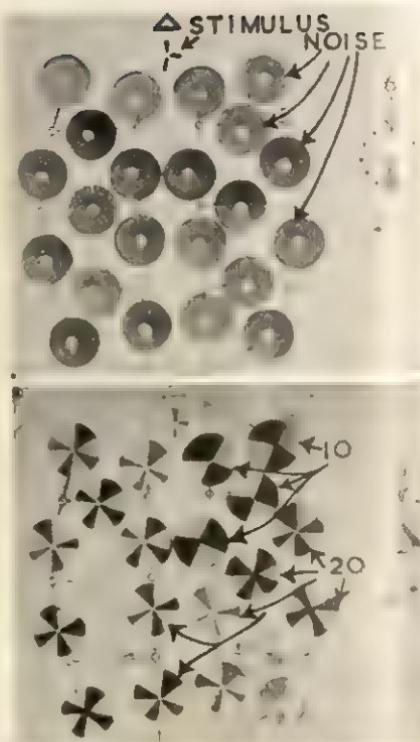
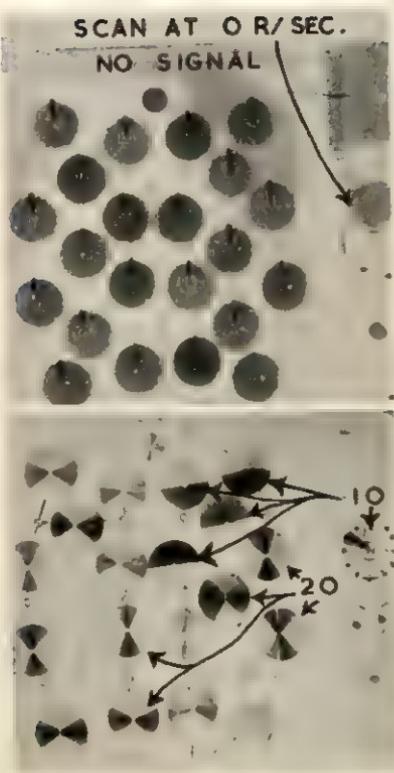


FIG. 11

FIG. 8A



FIG. 12



DISPERSION AND PERSISTENCE

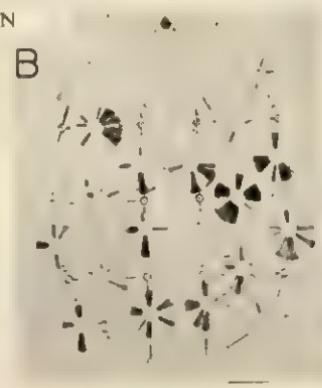
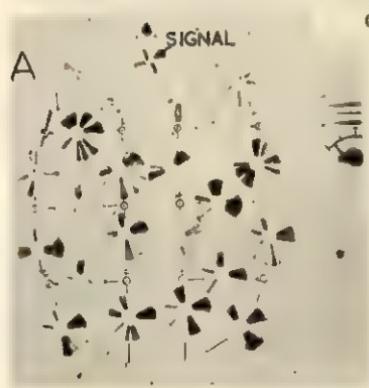


FIG. 13

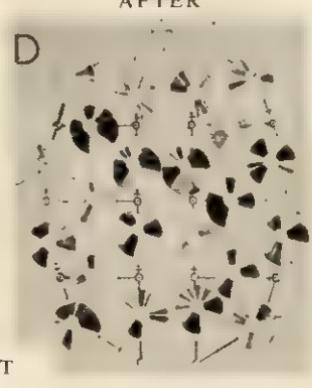
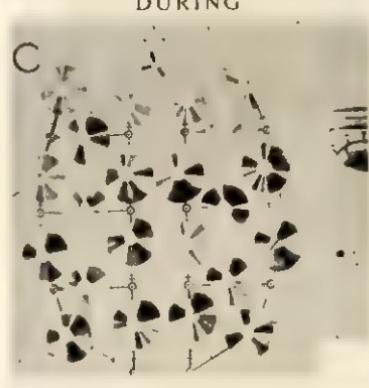
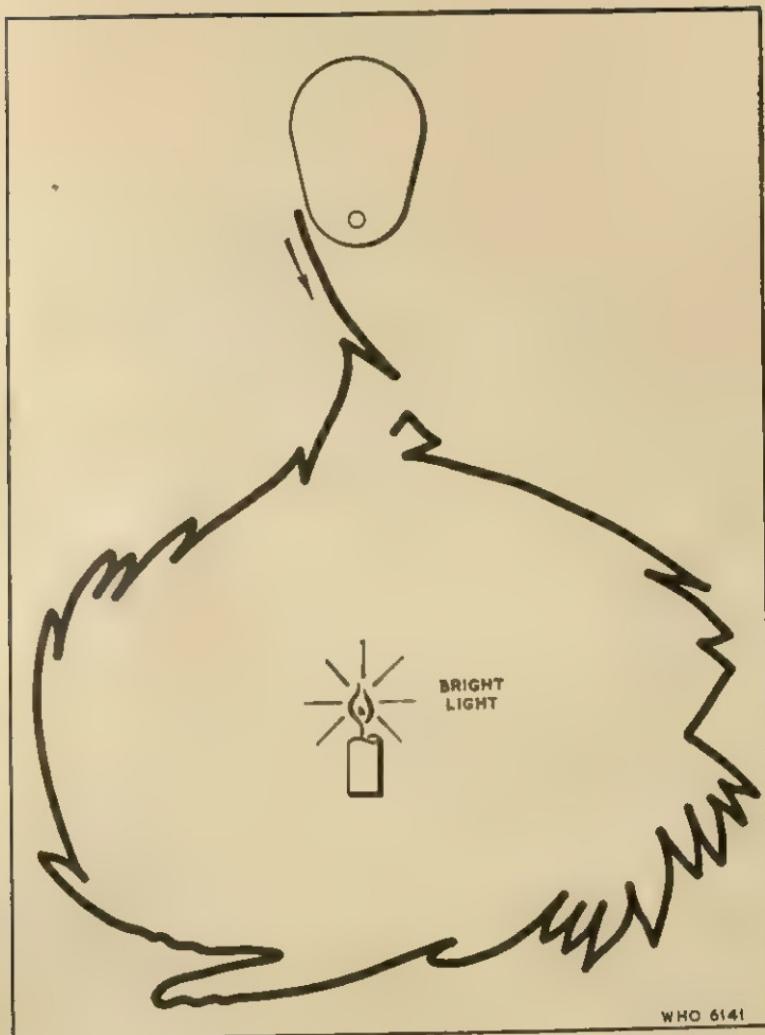


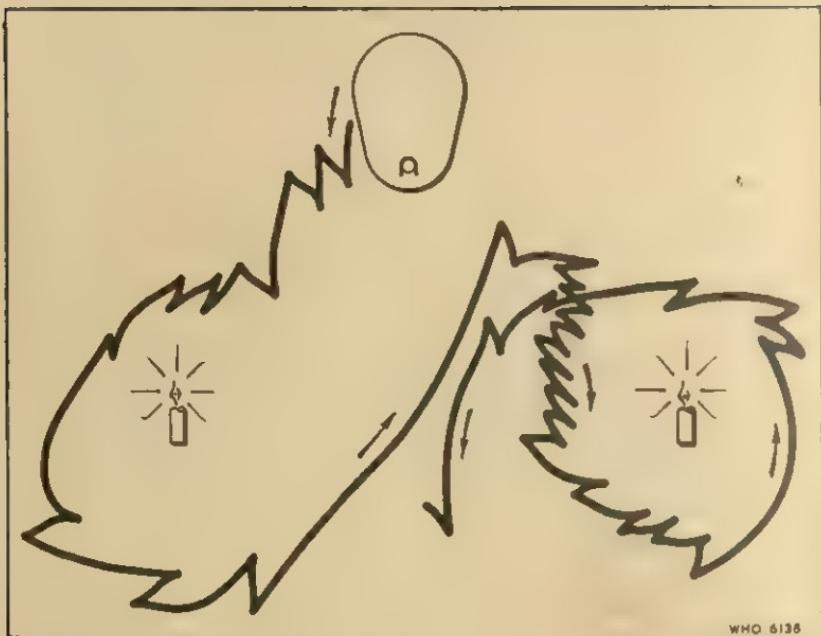
FIG. 4



WHO 6141

to seek an *optimum* and not a maximum. We have a very powerful attraction—a sixty-watt light bulb instead of a candle. The model starts, in this case, at the top of the picture and it chases towards the bright light source. But it reaches a point—the first squiggle—where the light becomes too bright. It then starts to circle round and maintains itself in an orbit of moderate brightness. It does not seek maximum illumination and run into the candle. An interesting point about this is that this moderation is attenuated, or even terminated,

FIG. 5

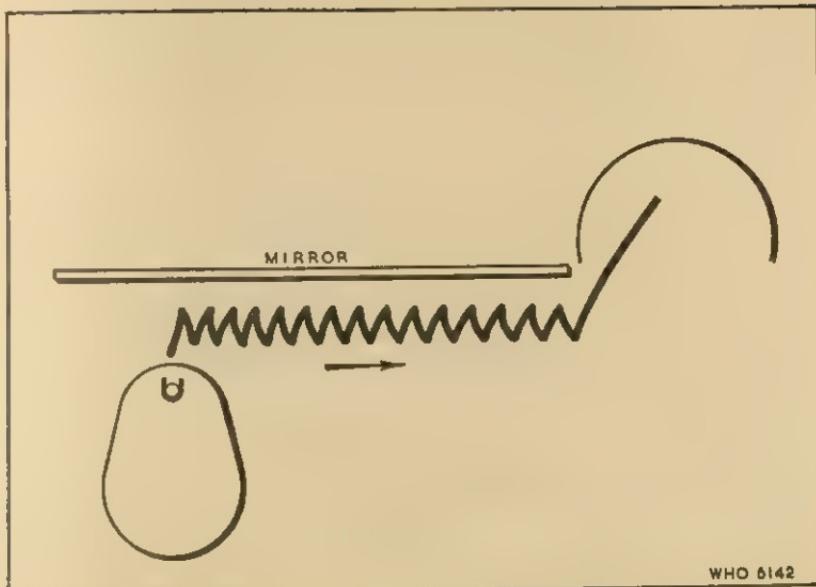


by appetite. If the creature gets 'hungry'—its batteries run down and it needs more electrons—then this extremely circumspect behaviour in relation to powerful stimulation begins to dwindle and finally it runs right into the light. This may have some relation to the behaviour of certain animals we know of who tend to become less moderate as their needs become greater.

Fig. 5 has a lesson for the philosophers among us, if there are any. This is the solution by a two-element model of what has been called the dilemma of Buridan's Ass. The dilemma, as conventionally stated, is that of a creature which does not possess free will, when it is faced with two exactly equal and equidistant stimuli; it is then not able to choose between the two alternatives and will die of hunger before it decides which of these two alternatives to accept as a stimulus.

This particular model has a scanning device in order to have freedom of movement. It also has a very interesting property in that it lives in Bergsonian and not Newtonian time. Newtonian time is reversible; the Newtonian solar system could be run backwards and it would be exactly the same as it is now. But Bergsonian time cannot be run backwards; all organisms live in Bergsonian time; time for us, as we all know, has an arrow. That arrow points to the grave;

FIG. 6



WHO 6142

that may be deplorable, but it has one important advantage, that with it we can solve Buridan's dilemma. We have the creature again starting at the top of the picture. There are two candles which are about equidistant; they may be precisely so. The creature is released at the top and it happened to see the light on the left first. The spatial symmetry is not also symmetrical in time, so the effect of a scanning machine is to solve all dilemmas which involve symmetries in space. One of the signals must be seen first, and the one which is seen first is the one visited first. The creature explores the possibilities; when it gets close to the first light its moderation mode is invoked and it wanders across and does the same thing with the other light; it forms a figure-of-eight diagram. So our two-element model, which is essentially a reflexive mechanism richly inter-connected with a scanning device, can solve the dilemma of Buridan's ass, and would not die between sources of nourishment.

With Fig. 6 we come to some of the refinements which emerged only some time after these creatures had been made. This mode of behaviour and the next one were, quite frankly, surprising to us though, of course, we ought to have been able to predict them. Fig. 6 illustrates the situation when a creature of this type is confronted by its reflection in a mirror. It has on its nose a small pilot light, put in originally to tell us what was happening inside; it is so arranged

that it is turned off when the creature sees another light; that is, it tells us when the photo-tropic mechanism is in operation.

In this case, the light which the creature was allowed to see was its own pilot light in the mirror. In this situation, the act of 'seeing' it makes it automatically extinguish the light which it sees. The apparent stimulus light having been extinguished, it turns it on again, then off and so on, so that you get a characteristic oscillation. You can see how peculiar and regular it is by the zigzag going up the side of the mirror. This is an absolutely characteristic mode of behaviour, which is seen always and only when the creature is responding to its own reflection. This is an example of the situation I described in the second proposition, where the reflexive circuit includes an environmental operator; in such a situation you get a characteristic mode of behaviour which occurs always and only when the model is reacting to itself.

Now, put yourselves in the position of a biologist exploring an unknown island. He comes across a hard-shelled creature; when he puts a mirror in front of it, it behaves in a specific way. He would write a letter to *Nature* and say he had evidence of recognition of 'self' on the part of this mollusc, crustacean, or whatever it was, because, by the rules of scientific interpretation he had observed a 'diagnostic' character.

Let us now apply this to a society of two individuals; only two because more than two is hopelessly complex. The two are placed a little distance apart on the floor and allowed to 'play'. Remember that each of them has a pilot light in its nose; in other words, each can see the other. Remember also that in seeing the other's pilot light it turns its own off, and you have the situation in which each of the creatures is capable of seeing the other, but in doing so becomes invisible. Then you have a very interesting waltz. The two circle around one another at a respectful distance; they never quite escape, nor can they ever consummate the attraction, because the moment they actually touch, the touch mechanism is put into operation and they recoil from one another only to be re-attracted. Just to demonstrate how like they are to certain societies with which we are familiar—I turned on the feeding light during the middle of this social pattern. The immediate effect of that was to break up the social pattern and instead of co-operative pattern-weaving you got competitive straight lines.

These are all unitary systems, which are the simplest possible devices which could behave like this at all. You see here evidence of some rudimentary trace of social organization, sensitive to a stimulus which breaks up the social pattern by attracting the individuals, changing them into a hustling line of people waiting to get into

the feeding-trough. Note too that in a condition in which the need for nourishment is small, the competition would be less. In other words, if these animals' batteries were very well charged, they would display moderation, would not get too near the bright light and would carry on the social routine in reasonable security, even in the presence of a bright light, with only occasional interruptions. But the moment they began to get fatigued then they would start to be fiercely and mercilessly competitive.

These pictures are designed to suggest the various possibilities in this type of study as applied to the simplest elements and rudiments of organization at the lowest level. These models illustrated so far have no memory and no power for social adaptation. They have reflexes, but no instincts. They have simply two reflexes with two receptors. They are self-maintaining, but they could not learn the best places to find food; it would be a matter of trial and error on each occasion. They are unitary, not plenary systems, and they are in every way as simple and as basic and, I hope, as easy to understand as they can be. They could not be any simpler and be of interest; if one took out one element or receptor they would lose any resemblance to a living creature.

(*Machina speculatrix* was then demonstrated.)

FREMONT-SMITH:

If we came upon this animal in the desert, how long would it take to understand it?

GREY WALTER:

Several of its generations unless you could dissect it. It is an interesting point of analysis that one cannot tell exactly what has happened inside it. You would have to kill it, work out its anatomy, dissect it and then see. It is interesting that when put in a pen with one way out it will always find its way out, by trial and error, though it has no intelligence, no insight and no cognition. One cannot predict its behaviour exactly, one can only categorically say that it will do such and such a sort of thing, one can define the end but cannot predict the means. As a corollary to that, no two creatures are exactly the same, even if one makes them as much alike as possible in components.

FREMONT-SMITH:

Why would not two behave exactly alike?

GREY WALTER:

They are so richly inter-connected. The slightest imperfection or variation is amplified vectorially, so you could start two at exactly the same position in relation to light and so forth and every time the scanner turned round in one it would be a micro-second sooner or later than the other's, and that would bring it to a new position and the differences would multiply cumulatively.

BOWLBY:

Why does it keep going round?

GREY WALTER:

It is looking for more lights. If you imagine a world which this thing is adapted to, and in which there are charging devices for its batteries, it would be important for it not to get stuck on one; it has a much better chance of being fed when it has several sources of food. It would wander around the room and always be near feeding sources. It is always looking for better and better light; it is never satisfied. If you made one of these things adapted to live in a tropical climate, you could make the shell of photo-electric cells and it would charge itself in the sun. It would be important for it to find a place where the sun's light was bright and steady, not between trees or houses, but a wide expanse of light; it would always seek this out and then sit and charge. At night it would have to collect dew-distilled water, so it would find two places, one where it could collect light and another where it could collect dew for its batteries; that is all it needs.

FREMONT-SMITH:

How many circuits are there in it?

GREY WALTER:

In this one there are two. The construction and circuit diagram are given in Fig. 7.

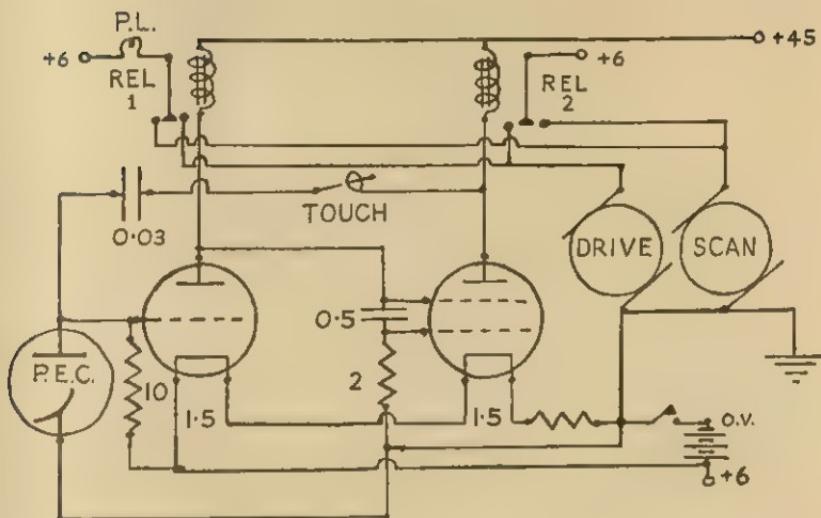
FREMONT-SMITH:

How many units are there in each circuit?

GREY WALTER:

Just one, but the systems have the capacity for inter-connexion, so that we may have either A, or B, or (A and B) or $A \rightarrow B$ or $B \rightarrow A$ or $A \nleftrightarrow B$. We have six modes of behaviour, excluding zero.

FIG. 7
THE ELECTRIC CIRCUIT OF *M. Speculatrix*



BINDRA:

What is its name?

GREY WALTER:

This one is called Olga, the beautiful spy. This spying machine was made for this type of demonstration with a transparent and rather glamorous coat, and a well-proportioned body!

BOWLBY:

It is sensitive to light and negative to touch, is it not?

GREY WALTER:

It does not see the light when it is touching something. The whole carapace is mobile and is the touch receptor.

LORENZ:

Could you explain the mechanism to us in a few words?

GREY WALTER:

Yes, quite easily. The batteries, which supply its motive power, also provide a grid bias, the voltage of which holds the valve just on.

When the batteries go down, the bias goes down and so the mode of moderation which requires both valves to work disappears.

LORENZ:

The threshold is determined by a summation between the stimulus received and the 'internal stimulus' of its present grid tension? When the grid tension is very low, even the addition of a very strong light stimulus will not be sufficient to reach the threshold of its 'turning away reaction'. Have I got this right?

GREY WALTER:

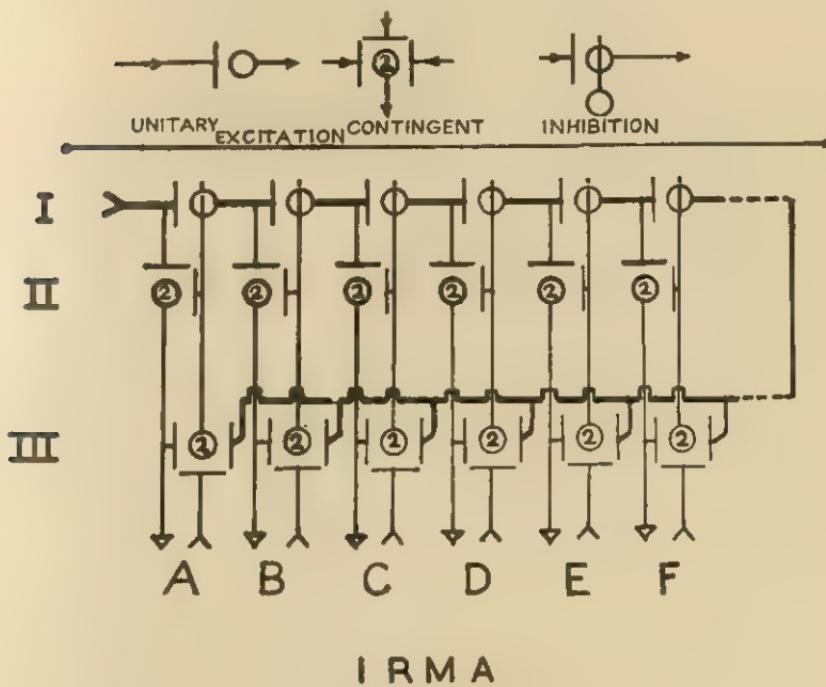
Yes. When the shell touches something the switch is closed which connects the output of the amplifier back to its input and produces a multi-vibrator circuit. The sensitivity to light is lost, and the steering-scanning motor is alternatively on full and half-power and the driving motor at the same time on half and full-power. The effect of this is to produce a turn-and-push manoeuvre. The time-constant of the feedback circuit is selected to give about one-third of the time on 'steer-hard and push-gently' and two-thirds of 'push-hard-steer-gently'. This seems to give a prompt response to the first contact with an obstacle, a reasonable chance of getting through a gap and a short after-discharge to ensure final escape. Though there is no direct attraction to light in the obstacle-avoiding state, the feedback time-constant is shorter when the photo-cell is illuminated, so that when an obstacle is met in the dark the drill is done in a leisurely fashion, but when there is an attractive light nearby the movements are more hasty.

From now on we get into very much deeper water, because we now embark on the subject of adaptation by imprinting, by instinct and by association. When Lorenz was with us in Bristol about a year ago, I said that when we next met I would have a model of imprinting to show him, and I have brought one with me. One difficulty is that the definition of imprinting, and its discrimination from other types of behaviour, is still a bit difficult. However, I have made a model which Lorenz may say is not in fact a model of imprinting at all, but it is a model of something, and it has some interesting properties.

Originally I intended to attach it to this moving toy, but the situation gets incredibly complicated when you have a model of imprinting, or, as you shall see later, of learning by association, actually moving round the room. It is as bad as bringing a small

FIG. 8B

THE 'NEURONIC' CIRCUIT OF I.R.M.A. THE HEAVILY-DRAWN LINE REPRESENTS THE CIRCUIT MAINTAINING SUB-THRESHOLD TONIC ACTIVITY AT THE INPUT CELLS IN LAYER III



kitten or sheep into the room and trying to explain what it is doing. So I detached the learning apparatus from the moving model and what I am going to show now are preparations rather than complete working models. Making a model of imprinting was not at all easy; it had to be simple enough to be instructive yet close enough to the original to be convincing. The model I finished with is certainly sufficient to show some of the features of instinctive responses and particularly of imprinting, but whether it could be simpler, I am not yet sure. Whether it is the only one that would work I do not know either. It is up to other people to make and play with similar arrangements, but it has two or three secondary characters which were unexpected, and which I think are interesting, for they suggest experiments to see whether in fact in any animal systems of this sort exist.

Before I show you the actual model, I think I will show you its neuronic circuit. (Figs. 8A, facing p. 32 and 8B, above.)

This model is called I.R.M.A. It consists of two networks which are arranged, for convenience, in three layers labelled I, II and III. This might conceivably be helpful in neuro-anatomical analysis, because it suggests that for instinctive, innate releasing mechanism (I.R.M.) type of behaviour one should look for a minimum of three levels, or three ganglia or layers of functioning elements. The essence of this system is that it contains one cascade chain of neurones or functional elements in layers. At the top are the conventional signs used in making up these neuronic analogues of electrical circuits. Top left is the symbol for an ordinary unitary excitatory synapse, a bar and circle, which is easier to draw than the conventional reversed arrow.

A contingent unitary synapse or junction is one which can respond to or transmit an impulse only if it receives simultaneous activation from a certain number of sources. The number drawn in the middle of the nerve-cell body, so to speak, is the number of excitations it must have to respond. In the example at the top there are three dendritic arborizations, as it were, round the nerve-cell, of which any two must be active for it to respond. Inhibition is indicated by the tendril of the nerve fibre actually going across or through the nerve-cell of the synapse.

In the circuit itself you will see that there is in heavy black, a chain of synapses all excitatory, which ends up with a very long process, an 'axon' if you like, which provides activity for all the cells in layer III. There could be an indefinite number of such elements in layer III though I have included only six. So we have a series or cascade of neurones leading to a large chunk of nerve tissue on the input side of the nervous system. The inputs are indicated by arrows. As it is a formal model, the receptors are simple push-buttons, though they could perfectly well be photo-electric cells or microphones, or whatever you please. The output to the reflex response system can be imagined as terminating in an effector. This model is so arranged that when the system is born, that is, turned on in the electrical sense, nothing whatever happens at first. But if a signal arrives from outside, that is if one of the buttons on any of these circuits is pressed, it has two effects. It activates its own circuit, which is already receiving one unit of activity through the cascade system, and requires two units. This unit of afferent activity plus the unit of 'tonic' activity succeeds in exciting the nerve-cell. This then immediately inhibits the corresponding cell in the chain in layer I, and activates the cell in layer II, and in so doing, it turns off the tonic supply from all but itself, and diverts the whole supply to itself.

This is the basic principle of the system; when it is turned on at low level, if nothing happens outside, nothing happens inside. But

if you press a button, then that one immediately has a unique effect, and the effect of all the other buttons thereafter is minimal. They may still produce some reflex action, but the effect upon the total activity (which I define simply as the amount of information circulating in the nervous system) is great only if the original button is pressed. If you came into the room and did not know which stimulus had been given first, you could discover this by pressing one button after another to find which one has the exaggerated effect.

There are two interesting secondary properties of this system, which I think are relevant, because they seem to remind one of the effects which Lorenz has described. The first is that this system will be extremely sensitive to the action of very slight perturbations of its metabolism. The reason is that the provision of a number of elements in series provides for amplification of effects, such as those, for example, of a drug. Consider a drug which in a given concentration has been found to reduce the probability of passage of an impulse across a single synapse by 50 per cent.; any impulses which do get across the first synapse will also have one-half as the probability of getting across the next one; and so on. When the final response depends upon transmission through a cascade system of this kind, any agency which reduces the chance of activity to one-half will have an effect on the whole system of one-half to the power of N , where N is the number of elements in the system. The action of a drug which has the moderate effect of reducing the level 50 per cent. in a single element will reduce activity over a thousandfold in a system which has a cascade of ten.

I remember Lorenz (Vol. I) describing the case of shrikes. The effects of very slight dietary deficiencies in these birds are said to modify profoundly one or two features of their instinctive response to their prey—they fail to impale it on a thorn. The behaviour of domestic animals may in some cases differ from that of wild animals because of a change in diet which is quite unmeasurable by biochemical means. A very slight change in metabolism might produce a dramatic change in instinctive behaviour.

The other secondary effect is that if the total activity in the system rises, you are liable to reach a state where the characteristics of the system are completely reversed. That means that instead of being an imprinting model it becomes a model of fashion, it follows the latest novelty. This might be interesting in relation to growth and the critical phases of development. As you turn up the power in the model it comes to a rather delicate phase where it is not quite sure whether it is imprinted or not, and as the power gets very big the imprinted response is submerged and a wide repertoire of responses appears. This change occurs without any circuit rearrangements. I

think this is an important point; in all attempts to analyse behaviour in neurophysiological terms, we should see whether the effects observed could be due only to change in magnitude.

FREMONT-SMITH:

Could you explain why a certain increase in magnitude leads to this change in behaviour?

GREY WALTER:

In this particular model, you mean? The reason is that when it is just turned on, the amount of activity available is only just sufficient to maintain the tonic activity and then to operate the chosen circuit.

FREMONT-SMITH:

That drains off all the activity?

GREY WALTER:

Yes, and when I provide more by increasing the current there is ultimately a surplus of activity which allows it at some critical stage just to hold a second response for a moment.

FREMONT-SMITH:

Can growth in this situation be represented merely by an increase in power?

GREY WALTER:

Yes, a simple increase in current in the 'tonic' network. Growth of the nervous system might well lead to an increase in the total number of impulses circulating in the nervous system. Information is a better word, perhaps. The more information there is, the less restricted the system is in its responsiveness.

MEAD:

When this becomes a creature of fashion, does the primacy of the first button completely disappear?

GREY WALTER:

Practically completely. This possibility of reversal or modification with change of size is one of the things which one has always got to consider. As the child grows older the brain physiology changes in

size as well as in kind, and it is not always easy to decide whether the change one observes is a quantitative or a qualitative one.

TANNER:

I take it that what you are calling the change in size as the child grows is the change in magnitude of the power input, rather than change in number of neurones. During growth there is no increase in the number of neurones after a very early stage, but an increase in the size of each and in the concentration of substances of one sort or another within them.

GREY WALTER:

The increased number of *active* neurones may be what matters.

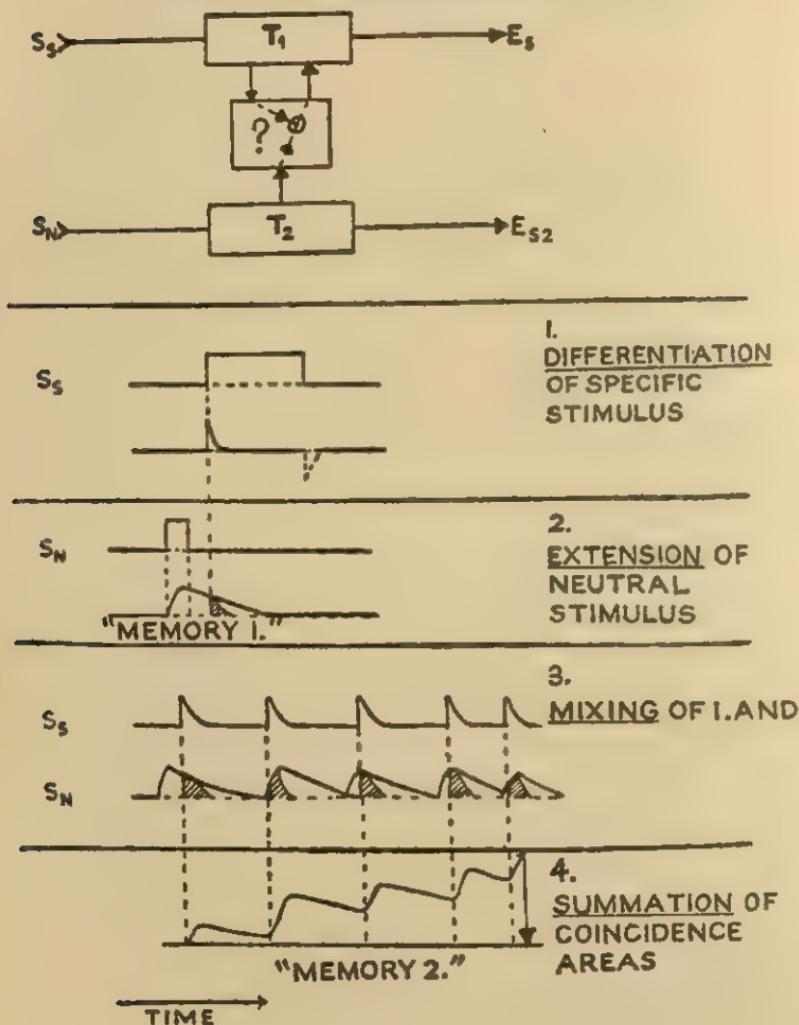
FREMONT-SMITH:

There are units actually active at any given time and units potentially active as well as units which are not yet even potentially active in the growing child.

GREY WALTER:

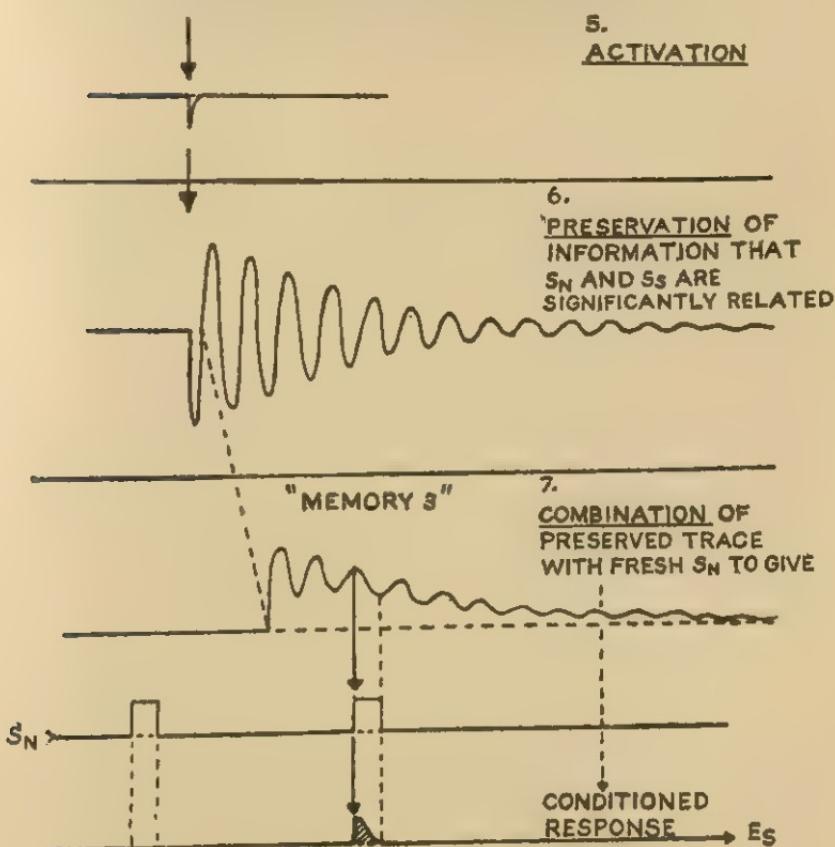
The thing I have more in mind is that, in real nervous systems that we know of, there is always some apparently 'spontaneous' activity. That corresponds in this model to the 'spontaneous currents' which flow when I turn it on. There is a battery providing the basic tonic activity, a current through the heavily drawn circuit in the diagram. A nervous system which contains neurones in this sort of circuit has the peculiar property that in early youth it allows for a very specific response to be built up, and later allows for a variety of response, without any anatomical transformation, simply perhaps with an increase in the frequency of the rhythmic activity—which does occur in growing children. This is close to my heart, because in the growth of the child's brain the most dramatic feature is the rise of frequency of the spontaneous activity, which must account for a large part of the total electrochemical energy available. It is just conceivable that some of these features might be related to a necessity in evolution for the development of infantile, instinctive, imprinting behaviour which can still become, in the vertebrate, a versatile repertoire of behaviour modes without any metamorphosis. In most insects there is a dramatic metamorphosis, in which behaviour seems to be transformed in some quite miraculous way, but in the vertebrate there is no possibility of metamorphosis, and there you may have to allow for the needs of the animal by this type of development during growth.

FIG. 9A
THE FIRST FOUR OF THE SEVEN OPERATIONS OF LEARNING—THE SELECTIVE OPERATIONS



Now we come to the question of learning by association. Fig. 9, A and B shows the learning-by-association situation as I should like to represent it for my own purposes; it is not very different from those which have been suggested before. There are two transmission systems: T_1 is exactly like the one in the first model I showed you, *Machina speculatrix*: a simple reflex system, an input with a specific stimulus S_s leading to a synaptic relay, which may contain two

FIG. 9B
THE LAST THREE OF THE SEVEN OPERATIONS OF
LEARNING--THE CONSTRUCTIVE OPERATIONS



synapses, and having a specific effect, which I have designated E_S . The specific stimulus would be, in the Pavlovian case, food in the mouth, and the response would be the flow of saliva; or S_S might be an electric shock to the leg, E_S its withdrawal.

Below it is another transmission system of the same type, but with a stimulus neutral to the response E_S ; in Pavlovian terms, the conditioned stimulus. This operates through another transmission system and of course may have a specific effect of its own, but with that we are not at the moment concerned. In the Pavlovian sense, supposing the neutral stimulus is a bell, there might be a pricking of the ears in response to the sound, but there would not be a flow of saliva. In respect of E_S it is neutral.

LORENZ:

While it is specific in respect to E_{S_2} ?

GREY WALTER:

Yes, as a reflex system it would have its own pathway, but it is not directly linked with the first system. But between the two we have a 'learning box' in which there is some sort of mechanism that provides for the building up of an association between neutral stimuli and specific stimuli in such a way that after regular and frequent repetition of a neutral stimulus followed by the specific one, the neutral stimulus comes to have the specific effect.

When trying to make a model of this some years ago, I got into considerable difficulties. I thought at first it would be easy to do, but when I looked at my own data, and the data of other people as to how conditioning really happens, I found that the performance specifications are complex and rigid. For example, the neutral stimulus must occur before or during the specific stimulus. It is no good ringing a bell for your dog after it has had its meat. Thus arises the importance of temporal order. There are several other such specifications.

Now as to the 'learning box' I established to my own satisfaction that for the functioning of such a system seven internal operations must be performed. It amazed me that a thing so simple as this rudimentary process requires no less than seven quite elaborate internal operations in the machine or in the brain. There may be more, of course, but I maintain there cannot be less. You will see that they have a definite relation to the learning situation.

The first operation is comparatively simple—the differentiation of the specific stimulus. The important thing about this specific stimulus is its beginning. The important thing which the animal has to detect in the giving of food is the first appearance of it. The way that is done here is to differentiate in such a way that we see only a pulse of activity at the beginning of the specific stimulus (see Fig. 9). In the case of the neutral stimulus, on the other hand, a different process must occur. The neutral stimulus must be stretched or extended, because there must be some degree of memory or storage of information of it. If, for example, the neutral stimulus is a bell just before the presentation of food, there must be something in the nervous system which stores at any rate for a short time a trace of the fact that the bell has rung.

The next operation is a rather more complex one, because it involves the mixing together of the abbreviated specific stimulus and the extended neutral one. They must be superimposed. This is represented in Fig. 9 by a series of specific stimuli, each one preceded by

a neutral stimulus with the areas of overlap of the two events shaded. It is those areas which are important. They are the areas of coincidence between the specific and the neutral stimuli.

That leads to a fourth operation, which is the summation of these areas. Each time a pair of events occurs together, the degree of coincidence must be recorded and stored. Here is a second type of memory. Our first type is simply a sort of after-discharge such as you find even in the spinal cord, but here we have a different sort of storage, a storage of areas of coincidence, which is a much more elaborate process, because it is not a storage of a single event, but a storage of association of events.

Note that these four operations are *selective* or *classificatory* operations. They provide for the selection of information from the outside world on the basis of contingency or coincidence. This is a very much more elaborate and interesting process than the instinctive response. This process of classification and selection is precisely equivalent to the recognition of pattern. You remember at the last meeting (see Vol. I), I mentioned that a system (whether flesh or metal) which could recognize pattern could learn and, conversely, a system which could learn must recognize pattern. Here the pattern is very simple, the pattern of contingency, of two things happening together in a regular and consistent order.

The next operation is what happens when the staircase effect reaches some threshold. When this happens it fires a detecting device which you will see indicated in the model. That then sets into operation a long-term memory. This is our third-class memory. This memory has an even more interesting character; it is the preservation of information that the neutral and specific stimuli were associated together *more often than would be expected by chance*. This is where we introduce the notion of a statistic of learning. In this selection and classification process we rely on some notion that there is a certain expectation of events and that the association of bell with food is extremely improbable to occur by chance. If they do happen together, bell-food, bell-food, bell-food, for a reasonable number of times, this is a phenomenon worth paying attention to. We recognize in this selective process and its immediate effect on behaviour what Pavlov sometimes called the 'go-and-find-out-reflex', the curiosity and the investigatory procedure which is the preliminary to all associative learning.

FREMONT-SMITH :

In the bell-food the food is something which the animal has had frequently in the past, but am I right in thinking that the bell is significant because it is new?

GREY WALTER:

Not necessarily; what is new is the association. It is not an essential condition that the dog should never have heard the bell before.

FREMONT-SMITH:

But if you had been hearing a lot of bells all along the line and getting a lot of food all along, would it not be much more difficult?

GREY WALTER:

Yes. If you make the statistics difficult to evaluate for an animal, if for example, it has heard the bell twenty-five times and had food fifteen times, then the establishment of the conditioned response depends, among other things, on how hungry it is.

The final operation is the combination of this storage or memory system with a fresh neutral stimulus to produce the conditioned response. We then get our impulse passing through the learning-box from the neutral transmission line to the specific one. Finally the modest pip on the bottom line of Fig. 9 is the conditioned response which has been produced by this elaborate procedure of statistical selection and construction and which requires these three storage systems.

It is very important from the philosophical standpoint to see in this analysis of learning that what is stored in the system is something quite unlike the original stimulus or the original response. It is a private image; it is an idea, a symbol. All that is happening in the actual model is an oscillatory discharge.

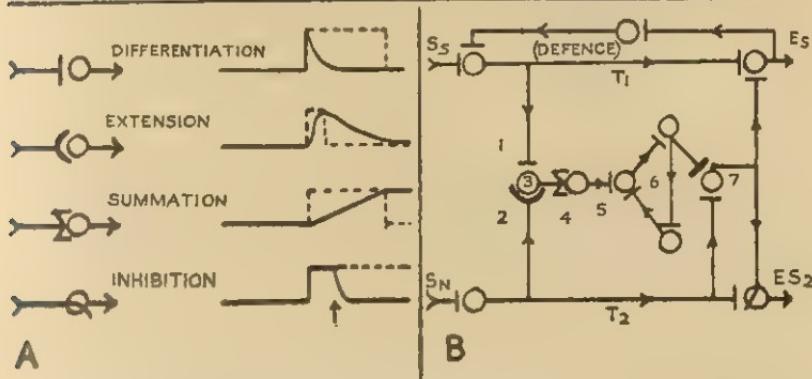
LIDDELL:

You are saying that every conditioned response is idiosyncratic or unique?

GREY WALTER:

Yes, that is one way of putting it. Fig. 10 is a neuronic diagram of the process. This is analogous to the diagram of the imprinting model, using the same conventional signs, the differentiating synapse, which is the commonest type in the nervous system, the extension or after-discharge synapse, and the summation and inhibitory synapses. For this particular analysis these four types of synaptic relay must be postulated. They are known to exist, in fact, in nervous systems and it would be rather nice if one could relate them to

FIG. 10



- (a) Conventional signs for indicating the four types of nerve junction necessary for learning, used in Diagram (b).
- (b) The simplest neurone circuit which could perform the seven operations of learning.

certain shapes and appearances of the nerve-cells. That is only possible in certain cases at the moment.

On the right we have the actual diagram of this system in terms of neurones or equivalent neurones. At the top we have the first transmission system, at the bottom the neutral stimulus, and between them the medley of neurones which are the learning-box. Our first process of differentiation is labelled (1). The second (2), the extension of the neutral stimulus, converges on the same neurone group. The effect of these two is mixed in neurone number three. The summated effects, when they reach a certain statistical threshold, activate the storage system, which I represent as simply a feedback loop.

A picture of this purely formal model is seen in Fig. 11 (facing p. 32). The display system is arranged so that I can show it to you as you would see an animal in the conditioned-reflex laboratory, but with some insight also into what is going on in its brain. There is a knob marked 'N', and when I press it a faint light comes on. This represents the neutral stimulus; it is faint because it has no significance at the moment. It can go to a number of places; it can radiate up or along. It has a very modest effect. It may have a specific action, but we are not concerned with this at the moment. Next there is the specific stimulus, with its reflex pathway, a fully-grown and perfectly conventional reflex. When I press the knob 'S', a light lights up, then another further along, its specific output. If I now press the neutral stimulus and follow it regularly by a specific one, there should gradually be built up inside

the model an estimate of the probability of things happening regularly in this particular order. You will see that at a certain point, after ten or fifteen repetitions, a little pink light will flash. Now we have our conditioned reflex established. I can extinguish it by not giving it any specific reward, but just the neutral stimulus. It will, however, reinforce quite quickly. The model has a latent memory even when it is not being operated. There is a knob labelled Reminiscence, by which you can make the memory long or short, as you like; the length of storage can be varied at will entirely for convenience. One of the interesting points about this storage system is that it will decay if not reinforced. If one establishes a reflex, and then does nothing at all, after a long time one finds the reflex has completely gone.

LIDDELL:

If you repeatedly give the neutral stimulus alone until extinction has occurred, and then press the reinforcement button by itself, can you get recovery of the response? You can with animals.

GREY WALTER:

You certainly can with a defensive reflex. In the defensive reflex, the conditioned response tends to be self-reinforcing. In Fig. 10 the defensive feedback circuit is drawn above the line T1. In this system, if you establish a defensive reflex instead of an appetitive one, it can be self-reinforcing, because every time the specific response is evoked by the neutral stimulus, it will look like a specific stimulus and have a self-reinforcing action. A defensive reflex may be self-reinforcing, or even, as it were, obsessional, and extremely difficult to destroy. In an appetitive impulse I would say it is more difficult to produce a reinforcement after extinction.

One of the odder effects with this device, which we observed by accident, is that if one uses a rhythmic storage system in this process, then the application of a rhythmic stimulus can in unfavourable circumstances evoke an imaginary response. In other words, by giving rhythmic stimuli, you can produce a conditioned reflex to something which never existed.

LIDDELL:

All animals will show these hallucinatory reactions if you work with them long enough.

GREY WALTER:

The paper by BRADY (1954) shows the establishment of a conditioned reflex to illusory colour sensations produced by stimulating

the eyes with white flickering light. Under these circumstances most people see, for example, a brilliant red from time to time which is 'not there'. When a conditioned reflex to actual red light is well established, conditioned responses occur when an illusory experience of red is produced by flickering light.

Now we come to the various types of memory required for these processes. We have already noticed that there must be three types, each with its own characteristic time of build-up and decay. There is the after-discharge type of memory; the staircase building-up of summated actions; and the long-term storage of information about two things that have occurred regularly together. If one wants a permanent memory—and in most higher animals there is a very long-term storage—one requires yet a fourth type, something which carries a copy of the whole experience. I should say that eight operations are necessary for this complete theory of learning.

The question which is often asked is, 'Which, if any, of these memories or these impressions are associated with consciousness?' Very obviously the first two need not be; in fact, they should not have anything whatever to do with conscious awareness. If they were conscious activities one would be continually preoccupied with all kinds of trivial evaluations. It is conceivable that in certain psychotic conditions this may be the case. The only class of memory which could be usefully conscious in the vernacular sense is the third one, dealing with the storage of information about contingency. That could well be in some sense a real experience, an experience which you can describe and evaluate on its own terms. I do not say it must be conscious; a conditioned reflex may be established without the animal or subject being aware that anything is happening. One can learn to dilate one's pupils unconsciously, or secrete insulin or produce glycosuria without being aware of it at all.

I have done some work with a moving model equipped with one of these learning devices. The situation it had to solve was to get to its food and search around a stool in the middle of the floor. Its education consisted very simply of trying to teach it that sound meant obstacle, which in turn meant trouble. The schooling was to blow a police whistle and kick it. After it had been whistled at and kicked about a dozen times, it learned that a whistle meant trouble. We then removed the specific stimulus—the stool. The whistle was blown, and it avoided the place as if there were a stool there.

I was more ambitious. In England a police whistle has two notes which sound together and make a particularly disagreeable sound. I tried to teach it, therefore, that one note meant obstacle, and that the other note meant food. I tried to make this differential reflex by having two tuned circuits, one of which was associated with the

appetitive response and the other with the avoidance response. It was arranged that one side of the whistle was blown before the machine touched an object so that it learned to avoid that, while the other side of the whistle was blown before it was supposed to see the light. The effect of giving both notes was almost always disastrous; it went right off into the darkness on the right-hand side of the room and hovered round there for five minutes in a sort of sulk. It became irresponsive to stimulation and ran round in circles.

As you would expect, there are only three ways of alleviating this condition. One of them is rest; in this case that was sufficient, it was left alone to play around in the dark until the effect of all the traumata had died down and it found its way home in the end. Another method is shock, to turn the circuits right off and start again with a clean bill. The most satisfactory method for my purpose is surgery, to dissect out the circuit.

Those are all the models I want to describe. I should like to round off my contribution with some mention of conditions sometimes referred to as 'stress' in learning. In the laboratory situations which I have described so far the experimental procedure has been carefully designed to eliminate stress and to provide for the models or for the animals as clear a statistic as possible. That, of course, is the purpose of the conditioned-reflex laboratory unless experiments are being made on conditioned neuroses. The same is true of the classroom or of this room here, where we are isolating ourselves from noise and other stimuli, so that what I am saying to you can seem to have significance which it would lose if there were a dozen other people talking together, or a band playing, or a lot of children running round the room. The function of education in learning is to emphasize, sometimes to exaggerate, the statistical significance of the stimuli which are presented. The function of the teacher or the trainer is to provide a pre-selected statistic, which he presents to the child or the animal, in such a way that it cannot evade working out the correct contingency computation.

Now I am going to go back about twenty years to describe to you some experiments which I did under the direction of Professor Rosenthal, who was one of Pavlov's pupils. He came to Cambridge in 1934, I think, to start a conditioned-reflex laboratory there, and I was seconded to him as assistant. We started the training of animals, and then Rosenthal went back to Russia for a holiday, but I was not fully trained, especially as Rosenthal spoke very little English and I spoke even less Russian, so that we got our wires crossed quite often. In the experiments we were making it was necessary to provide an absolutely regular repetition of stimuli, rewards and punishment, over a period of many months to get the

animal ready for the experiments Rosenthal wanted to do. But when he left, I completely muddled what he wanted me to do, and I have put in Table 1 a condensation of the protocols of some of these experiments.

L: Light	F: Food
T: Touch	P: Pain
S: Sound	N: Nothing

CONDITIONING IN SECLUSION:

			L	T	S	
LF, TP, SN.	LF, TP, SN.)	F	6	0	$\chi^2 = 24$
LF, TP, SN.	LF, TP, SN.	(P	0	6	0
LF, TP, SN.	LF, TP, SN.	(N	0	0	6
SIX TRIALS, NO ANOMALIES)				$P < 10^{-4}$
						eighteen stimuli

CONDITIONING UNDER STRESS ('NOISE'):

LF	TP	SP	LN	SN	LP	LF	TP	TP	TN	SN	LN	LF	SF	TP
TF	SP	SN	LP	LF	TN	LF	SP	TP	*LF	SN	TP*	LF	TF	TP
LF	LP	SN	TP	SF	SN	LN	LF	SN	LF	TF	LF	LP	SN	TP
SF	SN	LN	LF	TP	TN	SN	TP	SP	LF	TF	TP	SN	TN	LF
SP	SN	TP	LP	TP	LN	LF	SN	SF	TP	SN	TN	TP	SN	SP
TWENTY-FIVE TRIALS, ALL ANOMALIES												75 stimuli		

	L	T	S	
F	15	5	5	$\chi^2 = 24$
P	5	15	5	
N	5	5	15	$P < 10^{-4}$
—	—	—	—	
	25	25	25	75

Table I

Condensed protocols of experiments on Conditioned Reflex formation in five dogs, which accidentally demonstrated the effect of statistical stress in promoting neurotic traits in certain types of animal.

Light is represented by L, touch by T, sound by S, food by F, pain by P, and nothing at all in the reward situation by N. The experiment which I was supposed to carry on is represented by the top table, light-food, touch-pain, sound-nothing. What mattered was that every day the dog should be shown a 60-watt bulb followed by food; he should be stimulated by touch, followed by pain, an electric shock; he hears a loud sound, followed by nothing. The sound was the hum of a fan, which was going to blow various gases into the chamber, which was the purpose of the experiment. This

would be repeated regularly until the conditioned reflex was established, which it very soon was.

A record of six days of experiment is shown at the top. On each day the stimuli were presented in their correct order. There were six trials, with no anomalies and everything perfectly regular. This Table may be more interesting if you try to put yourself in the position of the animal. Supposing you want to decide for yourself, you being inside the experimental box, what the outside is like, you might draw up a χ^2 contingency table, with three rows and three columns, the columns being light, touch and sound, and the rows being food, pain and nothing. You find you have six occasions in which light was followed by food, six occasions on which touch was followed by pain and six occasions when sound was followed by nothing, and no exceptions. If you apply the χ^2 method to this, you find that the probability of this association being pure chance would be 10,000 to 1 against, though the numbers are too small to use or need the test. So you would be quite right in assuming light meant food, touch meant pain, and sound meant nothing.

When Rosenthal went back to Russia I was left with these dogs and, not realizing just what I was supposed to do, I arranged the situation as represented by the *lower* table. What happened, in effect, was that I gave these stimuli in an almost random order.

There were five dogs, and you see that in the 25 days of these experiments all the presentations were anomalous. Not a single one obeyed the law L-F, T-P, S-N. There was only one occasion when the associations were almost perfect, but even then they were not in the right order; L-F, S-N, T-P. Now if you set up the χ^2 table for these trials, you find light was followed by food on 15 occasions, and sound by nothing on 15 occasions. If you work out the probability of contingency, you find again that χ^2 is 24, and the probability of the association being random was again 10,000 to 1 against. In other words, if you were in the experimental box, it would be just as reasonable for you to infer significant association in the second case as in the first.

On this actual occasion, one of the five dogs retained a quite reasonable conditioned reflex response system, two of them became inert and unresponsive, and two became anxious and neurotic; in other words, although the situation had the same statistical value in both cases, it did not have the same effect on the animals. They were being conditioned under stress. If you like, you can call it 'noise'; there was a high level of randomness in presentation which had the effect precisely as described by Pavlov and others of producing a variety of experimental neuroses. The dogs had been selected on the basis of their typology, and the phlegmatic dogs became inert and

passive; the so-called melancholic and choleric ones became agitated and anxious; and the one sanguine dog managed, even through this noise, to retain its conditioned reflex responses almost intact.

FREMONT-SMITH:

You are making the assumption that the relationship of the experimenter to the animals was either equal in all these experiments or non-existent? You can overcome completely the tendency to neurosis if there is the appropriate relationship of the animal to the experimenter or of the individual to the leader.

GREY WALTER:

I would say only in some individuals. That is a very personal characteristic in dogs and in men, though it may not be true in the simpler animals. In fact, the animal that Pavlov called a sanguine type did survive completely.

MEAD:

Did you like him best?

GREY WALTER:

Well, I do not like melancholic, fawning-type dogs. The sanguine dog was an independent sort of fox-terrier cross, and I found him much pleasanter to deal with. The melancholic ones were whippet types, the phlegmatic and choleric ones were of no known origin, rather large dogs.

FREMONT-SMITH:

Then it does turn out that the one that was stable was the one you liked best?

GREY WALTER:

I liked him best as an experimental dog, but I am not very fond of dogs in any case. I only saw them on experimental occasions. I was not responsible for their kennel management. But I would like to stress the importance of personal character in the extent to which the breakdown can be alleviated or postponed or mitigated by reassurance or leadership.

LIDDELL:

In our early days, one of my colleagues was much more successful in precipitating experimental neurosis than the rest of us. Then we

discovered, after he had left, that it was his nervous, anxious management of the animal which was communicated to the animal and hastened the onset of neurosis.

PIAGET:

First I should like to say what great pleasure it has given me to listen to Grey Walter's communication and to express my admiration for his analysis of the conditioned reflex. I shall only discuss the six categories of psychobiological development which Grey Walter dealt with at the beginning of his communication.

The first three of these six categories are concerned with mechanisms which are to a great extent innate, whereas the last three are concerned mainly with acquisition by experience. From the point of view of the psychobiological development of the child this gives a correct picture and I do not want to add a further category to the six proposed. I think, however, that there is another dimension to be considered.

All behaviour presupposes, apart from maturation factors, an acquisition through experience, either in the form of exercise or as direct acquisition. Experience and environment are everywhere presupposed. There are, however, in the development of the child two kinds of experience which are always more or less intermixed, but which can easily be distinguished on analysis. These two sorts of experience give two kinds of knowledge or two kinds of methods of acquiring knowledge. There is first the experience which I will call 'physical experience' or 'experience derived from the physical environment' which is experience in the accepted sense of the term—it is what everybody thinks of, while forgetting on the other hand the second category of which I will speak later.

Experience derived from the physical environment furnishes knowledge taken from objects and gives rise to what one might call an abstraction derived from the object: certain qualities of the object may serve as signals for associations or for conditionings.

I will give just one example: the concept of heaviness and lightness. Evidently, it is by experimenting with objects that the child, before he is able to speak, at the sensorimotor level, notices that there are heavy objects and light objects. Then this knowledge gives rise to associations. He discovers that heavy objects are generally larger and larger objects are generally heavier. You all know the illusion of weight. When you see two boxes of equal weight but of different volume you always expect the larger to be the heavier; this results from an effect of contrast. The illusion does not exist among the mentally deficient nor among very young children. It is produced

partly by a weight-volume association. This type of knowledge is acquired by abstractions derived from the object.

There is, however, a second kind of experience which I should like to stress. Experimentation here is also carried out on objects and can be carried out only by manipulating objects. But in this case the knowledge is not drawn from the object and the abstraction does not derive from it. The abstraction is derived from the actions themselves which are performed on the object, which is something quite different.

I shall give only one example, which is a bad one from the experimental point of view, but which is very nice from the symbolic point of view. One of my friends, a mathematician, ascribes his first interest in mathematics to an experiment he made as a child. I should like to emphasize again the fact that everything, even mathematical knowledge, presupposes experience; such knowledge is not innate; it presupposes an experimental construction, an acquisition, learning, but this learning is of a particular kind. It is an abstraction derived not from the object but from the action. To come to my friend's experiment: as a child—I don't know how old he was—he clearly remembers counting pebbles. He had ten in front of him and he put them in a straight line. He began counting: one, two, three . . . up to ten. Then he counted from the other end: one, two . . . etc., and he was very surprised to get to ten. Then he mixed them up, he permuted and he began counting them again and again he got up to ten. This was a miracle to him. He tried every way and always arrived at ten. This is a result which he could not yet deduce in advance although later on he became a great mathematician. It was the experiment that taught him that the cardinal sum is independent of the order. But how did he experiment? With pebbles, yes, certainly. He could have done it with bits of wood, and if he had been an atomic baby with electrons—although they would have been rather more difficult to count. Anyway the object does not matter.

Where did he get his knowledge from? Two ideas come in here: the idea of order and the idea of the cardinal sum. The order is not inherent in the pebbles; you can put them in any kind of order. It is action which introduces a definite order into the arrangement of pebbles. In the same way, the sum is not inherent in the pebbles; the pebbles have no property of being ten. The sum is again a product of an adding operation, of an actual action. These two ideas of order and sum are not properties of the object. The object always conforms to the action, on the macrophysical scale at any rate. But it is not from the object that the knowledge is drawn. Here it is drawn from an abstraction based on the action.

This distinction is very important from the developmental point

of view. When it is a question of simple common associations, easily recorded, etc., these two kinds of experience are pretty well on a par, but when it is a question of actual experimentation, that is to say when the child tries to verify something, to ascertain by experiment the validity of a hypothesis, it is obvious that these two types of experience are not on the same level. Experience derived from action is much easier and at about seven years the child can already create logical structures, starting with the conservation of sum, whereas physical experience, that is to say experimentation, comes much later.

The same holds good for social acquisitions. The Greeks achieved the logical, mathematical type of experimentation but not physical experimentation, apart from a few exceptions, as in the case of Archimedes. There is a very great lag between the two. Physical experiment, which seems to be simpler, always comes later, because it presupposes very refined precautions and methods of control, whereas experience where the abstraction is derived from the action is a simple co-ordination of the action leading to operational schemata, which is much easier.

Moreover I should like to emphasize the fact—although I do not wish to anticipate what Mlle Inhelder might want to say later on—that logico-mathematical experience often conditions the physical experiment. In order to interpret a physical property it is often necessary to have an operational schema. I should like to give an example in connection with perception.

We carried out an experiment which consisted in comparing the length of two lines, one vertical and the other inclined at different angles. This comparison is very difficult for the adult. For the small child, on the other hand, it is much easier. The child is able to make much more precise comparisons than the adult. Why is this? It is because after a certain age a perceptive space is organized according to systems of co-ordinates which presuppose patterns based on co-ordination of actions. A small child does not yet have this system. He sees things directly, independently of their orientation in space.

It seems to me that an important distinction has to be made here. Mlle Inhelder will come back later to the question of conditions of interpretation of experiment, in connection with Mr. Whiting's communication, but I just wanted to stress today the two types of experience which presuppose different modes of acquisition, and very probably Grey Walter will propose schemata for the two types.

GREY WALTER:

I think it is a very valuable addition to this classification that one should always bear in mind the difference between a pure sensory

impression and one obtained by, as it were, physical experimentation. There is a difference between just looking at objects, saying 'Because that object is larger than another one, it is probably heavier', and the additional information got from actually performing an action on the objects. The experience or learning derived from motor action is different in some respects from experience related only to the receptor system. The process referred to as action, I would suggest, is what engineers call an error-operated servo-mechanism; in performing an action such as counting pebbles or making some motor adjustment you are matching an internal image of the object with some action which you perform. From the error in the match, you obtain additional information which is easier to evaluate than the purely chance exposure to stimulation.

RÉMOND:

I wonder if, between these types of experience, there isn't a place for something intermediate, where there would have been no actual action, but voluntary and conscious reasoning. Obviously, when we perceive differences between physical phenomena an unconscious association can be created between different facts, and therefore our involuntary experimentation can take place without our having performed any action. But, between this and purposeful experimentation based on action, could there not be a place for voluntary active reasoning? Do you not think that deductive reasoning without action is another way of acquiring knowledge?

PIAGET:

I do not think that this exists: I think rather that reasoning is the product of internalized action. Reasoning is the result of a series of operations which were first actions; it is simply a reflective level of internalization and of symbolization, permitting reasoning, but originating at the sensorimotor level, before the development of language and inner thought. I do not think that your voluntary reasoning constitutes a category to be added to Grey Walter's six stages. It is a repetition on another plane, an internalization of mechanisms.

RÉMOND:

I thought of that because you said that the first phenomenon was much more difficult than the second. When it is possible to repeat, transform and modify experience through action, one is enabled to acquire a certain piece of knowledge much more rapidly and much

more thoroughly, whereas in the first case it is a thing that passes, it is something definite which one takes or does not take, which one grasps or does not grasp; it is not so easy to profit by it.

PIAGET:

Precisely, it is because it is a different type of acquisition that internalization in the form of reasoning is simple. In the fields of logic or mathematics we are able to reason and make deductions without reference to experience. It is not the same as regards physical experience. That is the great mystery of mathematical learning: you are very soon able to do without experiment and to carry out voluntary reasoning. But you are able to do that just because it is a different type of experience from the physical experience. You have experimented with your own actions and once they are co-ordinated you no longer need objects; you can continue symbolically, internally. There is a certain logical mathematical type of deduction which a child of seven has mastered, whereas physical deduction seems more difficult.

ZAZZO:

I wonder if there is not in your previous explanation an expression which might give rise to a misunderstanding in the discussion, perhaps because it was too hasty.

In order to define the second type of experience you spoke of 'action performed on an object' which leads to ideas of order and of sum. In both cases—I think you will agree with me—there is action performed on the object.

PIAGET:

In both cases there is action on the object but what differentiates them is the type of action. The knowledge can be derived either from the object or from the actions themselves which were connected with the object; that is the only difference. But there is always action on the object before representative internalization.

BINDRA:

I think there is considerable experimental evidence on the point that Professor Piaget has made. There is, for instance, the experiment of EWERT (1930). He wore lenses on his eyes which inverted the visual image. With these lenses on you might consider him to be in the position of a newly-born infant who has to learn to perceive

the visual world. Ewert found that the only way he could get about in his surroundings was by constantly making and correcting movements of different types; he had to make movements with his hands, legs and so forth, in order to 'see' his way. Gradually, as he improved and could see straight, these movements diminished. When after three weeks he took the lenses off, he had to go through the same process of learning to see correctly with the help of awkward movements of arms and legs.

GREY WALTER :

If you imagine a target-seeking projectile that is looking for its target, it has to make a change of course and see if it is more nearly correct. It is not only looking at the target but inspecting its own trajectory, making one correction and then another. It would be the errors in adjustment which would give the information. What you are saying, it seems to me, is exactly what the servo-engineer says when he demands that the error signal should be fed back to the input. In other words, in taking up an object and examining it, one is matching one's idea to the real object and seeing if in all respects there is congruence between the idea and the object. For all I know, just looking at a teacup, it might be a model of a teacup with no space inside. This question of matching is I think included in this general theory, because I am assuming that the animal with reflexive action is capable of doing it. The first model I showed you is constantly orientating itself to a course, and in doing so is acquiring information about the error, or the difference between its own course and the course it should take.

MEAD :

I think it is important not to assume that these are alternatives, but simply matters of proportion. In a great deal of the discussion that we have had on the growth of thought of children there has been a tendency to assume that the sort of thinking in which motor exploration is not possible is in a sense primary, and the sort of thinking where error correction comes in is secondary; in other words, that there is a progressive adjustment to reality in the child as it assumes more mobility and has more opportunities to test its experience. But if you look at cultures that have gone to extremes either of immobilizing or of giving mobility to children, then you can see extreme results of these two systems. An extreme model of the first is swaddling, in which the child has very little opportunity to test its visual experience by the use of its hands for perhaps the first year or year and a half; of the second, the practice of the

Manus, whom I have just been studying, where the adult forms himself or herself into legs for the child and spends the time going where the child wants to go, leaning over so that the child can reach what it wants to touch, etc. In the first case, we find a willingness of the adult individual to accept an untested version of reality very easily, and in the other case we find an absolute refusal to do what we would call 'imagine'. If you present a Manus native with a picture of something, he will say either, 'That is that', or 'I do not know'. All the nuances that are possible with early experience of uncorrected visualizing, for instance, are removed by this very early insistence on continually testing, holding, pushing, pulling of everything the child sees. In most people in our society we have a mixture of these two.

LORENZ:

I quite agree that the two types are very distinct from each other, and I was going to emphasize the point that Dr. Mead has just made about children of different cultures. There are animals (for example, plovers) which, in visual exploration of the external world, do not do anything actively: they just stare. If humans want to form an idea of an object, they will always do some active exploration, even if it is only moving their eyes, focussing one spot after another and turning their heads to and fro.

OTTO KOEHLER (1950), in his very profound experiments on teaching animals and birds to count, found the same type of transpossibility from one sense of modality into the other. He has trained pigeons to the number six. The pigeon will look at several little heaps of grains and then choose the one consisting of six grains, quite irrespective of the order. It need not necessarily be grains, it might as well be pieces of Plasticine stuck on paper in different sizes and forms: the number is the only thing retained. Then the pigeon can easily be trained to take six grains successively falling out of a chute: you can draw out the temporal sequence unrhythmically and the pigeon will stand there and wait for its sixth grain, and walk away unconcernedly if several more drop out.

Koehler has trained parrots to do a still more difficult thing. The parrot is presented with pasteboard cards marked with irregular blotches, varying in number from one to eight. The bird looks at them and then picks the corresponding number of grains and walks away. This is an absolutely outstanding performance.

Koehler made another most exciting chance observation on a jackdaw that had been trained to take six pieces of cheese out of a series of covered boxes. The pieces of cheese were distributed in an

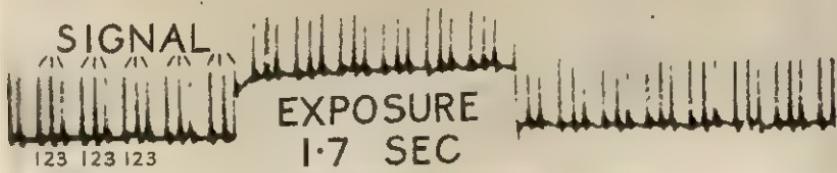
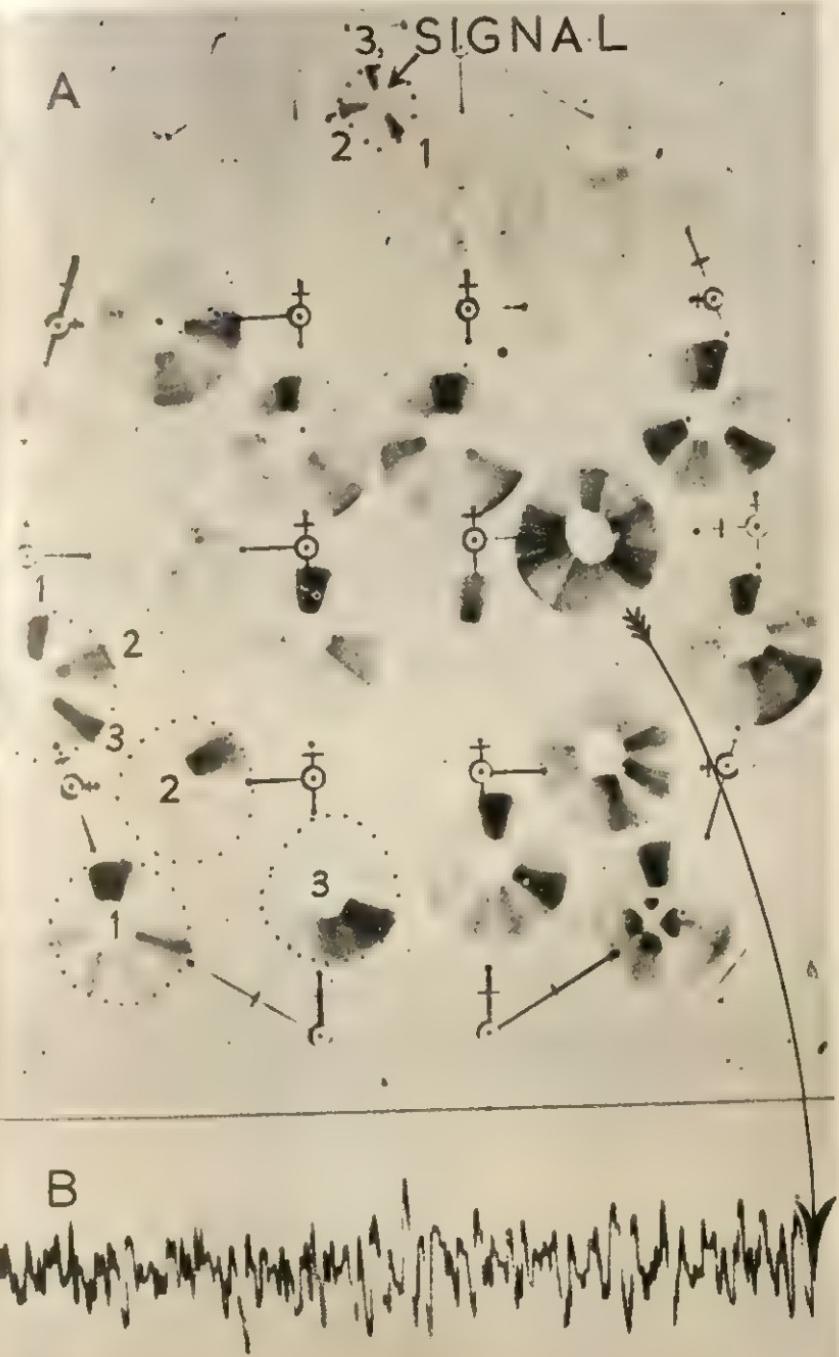


FIG. 15

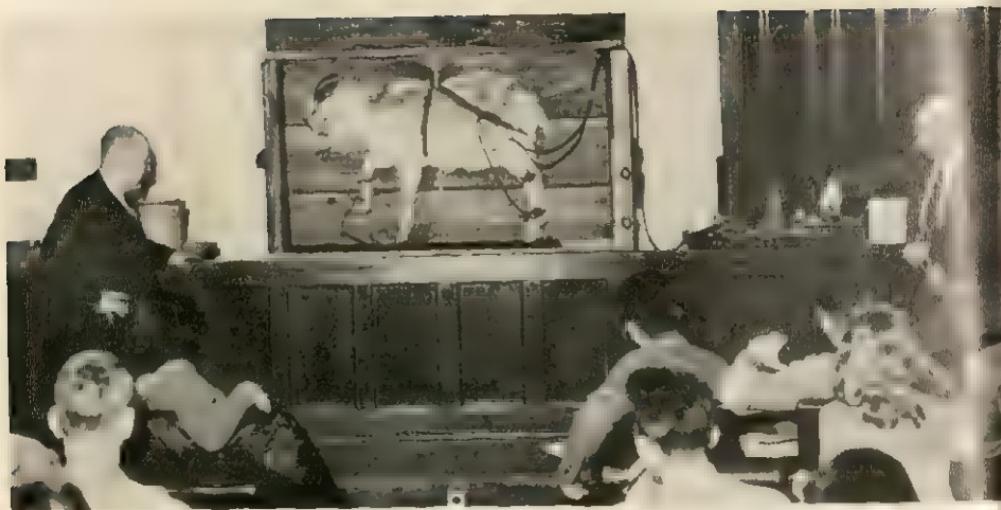


FIG. 19



ever-varying sequence; often there were several in one box, then again a number of boxes were empty, and so on. In the experiment in question, the bird found one piece of cheese in the first box, two in the second, the third was empty, in the fourth it found two more pieces, making five in all. Then the bird erred, thought it had enough and turned away. After a few steps it stopped, came back to the boxes, made one intention movement of pecking at the first box, two at the second, passed over the third, did two hints of pecking at the fourth box, after which it resolutely went on opening boxes until it got to the one containing the sixth piece of cheese to which it 'had a right'. It was exactly as if the bird said: 'Now let me see, there was one piece here, two here, etc. etc.' until it came to the conclusion 'Why yes, I was quite right, I have another piece of cheese coming!'.

GREY WALTER:

Now I should like to describe some observations concerning the way in which the human brain deals with the information that it receives, and the way in which such information is turned into the raw material of learning behaviour.

I am encouraged to show these records because several of my friends here have said that they feel this sort of approach to brain physiology is in some way a liberation from the strait jacket of Sherringtonian spinal cord physiology. These pictures show, for example, that the differentiation and specification of the brain areas, on which I think probably all of us were educated, though useful for certain purposes, represents only a vague simulacrum of the truth. The brain regions which are so dear to many hearts, though they may exist in a sense, are functionally very much more like the organization we have here today, where it is possible to say so-and-so is a physiologist and such-a-one is a psychologist and yet, although that may be their special role, at times they may be dealing with something quite different.

I should like to skip over the technical features. The input arrangements are very similar to those of the conventional electroencephalograph, but there is a special arrangement to display the electrical activity at the output. Fig. 12 (facing p. 33) shows this display system and indicates the reason for doing it in this manner, which is to show directly the pattern of the placement of the electrodes on the head. In all these pictures you will see a series of white lines and small circles. The small circles represent the electrodes and the white lines are the leads to the amplifying channels, and for each channel there is a cathode-ray oscilloscope.

The little circles are the electrodes, and at the top is the nose diagram, a triangle simply outlining the nose. The subject is lying on his back and you are looking at his head from the top, with the nose appearing at the top of the diagram. Each of these oscilloscopes carries a brilliance change, exactly as a television set does. In other words, the signals in the various channels are transformed into changes of brilliance on the little cathode-ray oscilloscopes like the television voltages which we see as brightness. In each oscilloscope there is a rotating scanning vector which is produced by the operation of the electronic beam inside the oscilloscope, and this line moves round the tube at a controllable rate. The whole arrangement therefore forms for us twenty-two clock faces. Each clock has one hand, and this hand tells us the time. The time it tells us may be our time, Greenwich Mean Time, or it may be brain time. In other words, we can make a part of the brain drive these clocks at its own speed. We may choose, for example, the bottom right-hand side, which is the right occipital channel; we may connect this to a special circuit which generates a speed in the scanning machinery which is synchronized with the brain activity in that region, and then our display will show us brain time. If brain time varies as compared with our absolute scale, then the rotational speed of our clock hand will vary, therefore we have a display which synchronizes what we see, not with our own clock time, but with the way in which time elapses for the brain in that part at that particular period.

The object of all this apparatus is to make possible the recognition of very small regular components for short periods of time, a second or so, against a background of interference or spontaneous activity or activity not related to this particular synchronized process.

Fig. 12 shows calibration records indicating the way in which each pattern is defined. Patterns with a single petal correspond to activity at the frequency of the scanning vector; they may have twice the frequency or three times or four times the frequency with 2, 3 or 4 petals. Therefore, one has a measure of frequency, phase relations, of latency in relation to absolute time, of position and, above all, the chance of extracting pattern from a mush of noise in the same way as a spectroscope can extract from a diluted and contaminated solution the spectral lines corresponding to a particular compound such as haemoglobin.

LORENZ:

If I understood it correctly, in the lower right picture those four flashes mean that these brain cells have a frequency which is just four times that of your hand going round. Is that correct?

GREY WALTER:

Yes, four times to one on the clock in that region. The frequency is read off the meter which says 5 so that frequency is 20. The point is that an irregular, random component, which does not appear on each occasion, is not recorded; unless it occurs on each occasion at the same part of the tube it appears only as a blur, if at all.

RÉMOND:

In that way the instrument is an auto-correlator?

GREY WALTER:

Yes, and a cross-correlator, because you have superimposition of signals in different regions, and the photographic method of recording ensures that the activity is integrated.

One point I want to emphasize is that from records of this type one can infer that alpha activity is a very complex process. There are many alpha rhythms which perform all kinds of functions. I do not think there is ever only one alpha rhythm. With any given individual it is possible to work out what the various alpha rhythms do, and very rarely does a given alpha rhythm component do the same thing for different people. Frequency is not a label of function. That is determined by other factors.

In Fig. 13 (facing p. 33) the subject is being exposed to a known stimulus. We have, on the left-hand side, at the top, the conditions during stimulation with a pattern of flashes with the eyes open. Below on the left we have the eyes shut; in each case on the right-hand side are the records taken after stimulation was terminated. The pattern of stimulation is important. Here it was groups of three flashes together at a group repetition rate of three per second. The pattern rate is represented on the top tube by flashes on our clock face; one appeared on our clock face at about twelve o'clock, one at about five, one at about nine.

During stimulation with the eyes open, one sees this pattern appearing in many parts of the occipital region of the head, as you would rather expect, near the projection or association area for visual function, and also in the anterior region of the temporal lobe, particularly on the right-hand side. The effects of the visual stimulation go far beyond the boundaries of visual projection, and even beyond the zone of visual associations, into the temporal region and into the frontal regions from time to time. We shall see later that the conditions of this spread are dependent on the attitude of the subject to stimulation—that is, not dependent simply upon the existence of his brain, but upon his history and his personality.

On the right-hand side there are records taken after the stimulation, and the thing to note there is that, when the stimulation is over, for several seconds the image of the response hangs on and persists. Notice that of these two, in the one with the eyes shut the persistence is greater, the reproduction of the pattern is clearer, and the spread is more extensive.

FREMONT-SMITH:

The flashing is through the closed lids?

GREY WALTER:

Yes. It is a very bright flash and even with the eyes closed it is still a very brilliant stimulus; but when it was over there was nothing to see—no competition from other signals.

BOWLBY:

How long after these stimuli were they taken?

GREY WALTER:

One second after. One can trace some effect for a period of five or ten seconds even in scalp leads. Of course the potentials are greatly attenuated by the skull and they must last for upwards of ten seconds in the brain itself; in psychotic subjects they may last as long as ten minutes. In other words, the pattern is preserved in the brain, not necessarily in its original form—there are signs of embellishments, abstractions, corruptions, and all kinds of transformations.

RÉMOND:

What is the duration of the integration?

GREY WALTER:

The actual exposures are all of the order of one second, so that if there are three repetitions here we have the superimposition of three rotations of our scanning vector which indicates the degree of constancy of this appearance.

WHITING:

How long does it take for the latency of the light patterns?

GREY WALTER:

There is a long latency. The actual latency for this particular pattern comes out temptingly close to the reaction time of the subject; in other words, not the physiological latency of 30 to 50 milliseconds, but more like 200 milliseconds. That is, it takes a fifth of a second for the pattern to reach the temporal regions of the brain. It takes much longer to develop physiologically, as a pattern; it builds up over perhaps a dozen exposures until this stage is reached and then each individual pattern takes another 200 milliseconds to reach the association areas of the brain. It is rather far from Sherringtonian reflexes—you see the brain acting not as a reflex system, but as an integrative and abstracting organ.

Fig. 14A (facing p. 64) suggests how this may be done. At the top, we have the input pattern. At the bottom again we have the response to the three flashes; in the occipital region we have the same pattern, but if you notice, it is not complete in any one region. We have flash number 1 in one channel, number 2 in another and number 3 in another again. This indicates that the first step in this complicated procedure must be the separating out of the components of the time pattern in different regions by some sort of sweeping or dissecting process by which, so to speak, the beads of information are strung on the string of time. The string and beads are sent through the post as it were to some other region such as the frontal or temporal, and then strung off again. I call this process abscission as compared with the complementary process, abstraction, where the beads are taken off the string. If you want to send someone a pattern of beads in a certain order through the post and you put them in a box they will be jumbled up, so you put them on a string.

LIDDELL:

Do the association fibres transmit the string of beads from the cortex?

GREY WALTER:

Not the long association fibres as far as we can tell; the impulses seem to dive down deep. The latency is too long for it to be a direct cortico-cortical process. After the pattern is dissected out, there seems to be reassembly again in a rather compressed form towards the temporal region. On the right-hand side it is reassembled in a rather expanded, idealized form and, in fact, there are six representations of this threefoldness in different parts of the brain. This is a fairly regular appearance, although it is rather unusual to get so

very clear a representation of the abscission effect; one has to take the exposure with the scan at a certain speed related to the alpha rhythms. There is every reason to suppose that this abscission process is one of the alpha rhythm functions; the first stage in recombination of a time sequence may be dissection by an internal spontaneous rhythm of some sort.

Fig. 14B is a record taken simultaneously with the previous one on a standard machine giving a written record corresponding to the left temporal channel, the one where you see clearly the pattern of threefoldness as the pattern is reassembled. The bottom channel shows the stimuli, in waltz rhythm. The raised part of this trace is the duration of the exposure of Fig. 14A, lasting about 1.7 seconds in this case. Such a record would have to be analysed by some very special means in order to extract the information of threefoldness that it represents. The noise level is high enough to obscure completely, even to the trained and eager human eye, the linkage of this particular abstraction. I think this explains why these effects are hard to observe and why one is bound to use these rather fancy devices to reveal them.

BINDRA:

What dimension of your actual brainwave is represented in your tubes?

GREY WALTER:

Amplitude is turned into brilliance, frequency is indicated by the pattern, and its position in the brain by its position on the display. In these records the amplitudes of the principal components were from 5 to 20 microvolts.

BINDRA:

Is there any way of your getting rid of any artefacts?

GREY WALTER:

Yes, indeed. Artefacts due to stimulation are seen as exactly synchronous with the stimulation and can be recognized easily, physiological artefacts such as those due to muscular activity are not synchronized with the servo device, since they are not related to the brain or selected by the operator. Most artefacts become noise; noise by definition is not synchronized or regular in its pattern, and, therefore, is either not seen at all or else seen simply as a vague blurring of the discs.

LIDDELL:

How does this brain work change from infancy to the adult?

GREY WALTER:

That really is a sixty-four dollar question. It is one of the subjects we are studying now, as carefully as we can.

LIDDELL:

I was thinking of Freud's dream work and the straightforwardness of the child's dream.

GREY WALTER:

The relative simplicity of the child's mind seems to be related to one of the odd things which we did not expect at all; that in young children these peculiar elaborate remote effects of stimulation do not exist at all. Up to the age of even three or four it is very unusual to see any sign of them. You get only a direct elementary response. As far as abstractions and associations go, the picture is extremely simple, and without any refinement. The remote elaborations do not appear until the age of six or seven, and then they start to develop very rapidly.

LIDDELL:

Do you not think there is correlation with the simplicity of the dream?

GREY WALTER:

The child must see things far more simply. The learning machinery has not matured.

Now, to conclude, I should like to describe how some of these results may relate to what we were saying last year about the social implications of some of these observations. In sorting some conventionally analysed records in our laboratory Janet Shipton found by chance that when the records were arranged according to their arbitrary typology, the cards kept coming out in pairs. Mrs. Shipton took the trouble to see in what pairs they came out and why, and oddly enough they were the very pairs of subjects as they had come up for the investigation. The people who chose to come to us in pairs as normal controls had electroencephalograms which had a marked resemblance. We are now trying to see if there is a cerebral affinity, so to speak, between people. In a provincial university town

with a mixed population of people of the same age, varied interests, free choice of companions there may be a tendency for people to choose companions who have similar brain activity, not of course because of that similar brain activity, but because they have personality traits which are associated with certain types of brain activity. Oddly enough, this association is best of all between the sexes. Two of these pairs of similar cards were those of engaged couples, and their records resembled one another almost as much as if they had been identical twins. This might throw light on human genetics. Selective mating would account for the continued existence after so many generations of brain types; otherwise one would expect brain characters to be normally distributed like stature.

LIDDELL:

It is well known that long-married couples become more and more similar.

GREY WALTER:

And adopted children become like their adoptive parents.

BINDRA:

I wonder if you had excluded all possibility of any gradual changes in your experimental set-up over the time.

GREY WALTER:

Yes. This observation is based on results from a consistent series of experiments over a period of about nine months. They were designed to study imagery, stereognosis and versatility, as I described to the Group last year.

BINDRA:

Did the subject come in at different times on different days?

GREY WALTER:

Yes. In some cases sociograms have been done on them by other people; wherever A admired B and B admired A, their records were of similar types. Where there was a 'star', somebody everybody admired, but who might have a reserved attitude to other people, there was often a peculiar sort of record. One might be able to plot these so-called sociograms in terms of physiological gradients. In evaluating these results one has to compute the possibility of

association of rare features; to reckon whether the E.E.G.s of these people may be associated by chance. We are doing this now, and so far it has been shown that the probability of such aggregation occurring by chance is small.

BINDRA :

It may be that two people come to the laboratory together. They have a conversation on the way and this conversation creates some sort of set in both of them. It is the common set that may be responsible for the resemblance in the two records.

GREY WALTER :

They do not know the nature of the experiment until they come. They are only undergraduate associations; they may be transient—many of them are—but for the time in which they are, they are important. They are people who join the same classes, have the same sort of recreations perhaps. It would be interesting to study just what the factors concerned are; whether they are determined by something more specific than accidental whims.

LORENZ :

What are the records of people with epileptic fits, do you find that their nervous elements tend to fall into step?

GREY WALTER :

Yes, that is the most dramatic of all these appearances, one finds absolute synchronization and regularity.

LORENZ :

That is an example of not saying the most important thing, because it is too obvious to yourself. I think you ought to have struck the table and said, 'When there is a fit, they fall in absolutely!'. This is absolutely conclusive, is it?

GREY WALTER :

Not quite for the general thesis, but it is very encouraging.

LIDDELL :

What about identical twins who have been separated for some time; have you ever run into that situation?

GREY WALTER:

They do tend to remain fairly close. One of the difficulties in applying electrophysiology to the problems we are discussing is that there is a very strong possibility that 80 or 90 per cent. of E.E.G. factors we can measure are genetically determined; and the other 10 to 20 per cent. are in some cases the most interesting. There are some cases for example in which neither the frequency nor the amplitude of the alpha rhythms change; but the response to stimulation does, very dramatically. These are individual cases and some can be modified by treatment.

As far as epilepsy goes, when only one of a pair of identical twins has seizures, the other usually has an 'epileptic' type of record.

FREMONT-SMITH:

If we were all in absolute agreement, would we have a mass convulsion?

GREY WALTER:

It might be very interesting; perhaps we should be convulsed with laughter!

FREMONT-SMITH:

Religious frenzies are perhaps in that category.

MEAD:

As among the people I have just re-studied, the Manus of the Admiralty Islands.

GREY WALTER:

I think the probability of our engaging in a religious frenzy is extremely small!

SECOND DISCUSSION

Presentation: Dr Bindra

BINDRA:

At the McGill Psychological Laboratory there are a number of human and animal studies in progress, dealing with a variety of psychological problems. Originally most of the problems were related in some way to Professor HEBB'S (1949) biological or neuro-physiological theory of behaviour; but by now, as often happens, the experimental work has proliferated so much that its relation to the original theory is not always obvious.

The first series of studies that I should like to talk about deals with the effects of variation in early environment on behaviour. Since this might lead us into a discussion on the whole subject of innate as against learned patterns of behaviour, I am going to present a scheme, originated by Professor HEBB (1953), which helps discussions on this topic. As psychologists, we are not concerned with the more or less biological concepts of 'heredity' and 'environment'. Psychologically, what concerns us is the distinction between 'innate' and 'learnt'. The terms 'innate' and 'learnt' are used in various senses and I think Hebb's scheme clarifies some of the confusion.

Factors in the normal development of behaviour: Hebb's formulation

1. *Hereditary*: the genetic (and any other) physiological properties of the fertilized ovum.
2. *Hereditary or Environmental*: chemical characteristics of the uterine environment of the embryo.
3. *Environmental: chemical*: nutritive influences of the post-natal environment, including oxygen, water, hormones, enzymes, etc. This refers to the environment of the nervous system, not of the whole animal, so this factor is summed up as the chemical constitution of the fluids bathing the nerve cell.
4. *Environmental: learning: sense-organ stimulation, species-predictable*: pre- and post-natal sensory events that are inevitable in ordinary circumstances for all members of the species.

Examples: moving the fingers in a particular way inevitably stimulates the palm of the hand (grasp reflex); first extensive contact with living organism is from parents and litter-mates. Such patterns of stimulation are the basis of the first learning.

5. *Environmental: learning: sense-organ stimulation, variable from one member of species to another.*

The basis of what is usually considered learning: where one can see that learning has occurred, because the animal that has seen or done certain things thereafter responds in a different way from his fellows.

Both factors (4) and (5) are learning factors, though (5) is easy to control for experimental purposes, while (4) is hard to control. Another important point is that both factors (1) and (2) are sometimes called hereditary factors, but actually they are not the same; (1) is genetic, (2) is environmental-chemical.

I am now going to discuss our experiments on dogs. This series of experiments is concerned with the role of early learning or early experience in emotional and in problem-solving behaviour. These experiments were conducted by three investigators—THOMPSON and HERON (1954a, 1954b), and MELZACK (1952, 1954). Since it was a study of the extent of variation in behaviour that could be produced by changes in early environment, the genetic factors were kept constant as far as possible. All the animals used were pure-bred Scottie dogs, and in most of these experiments all the dogs were the descendants of a single pure-bred couple. At the time of weaning—that is, at four weeks—each litter was divided into two groups, a control group and an experimental group. The animals in the control group were raised as pets in fashionable homes in the Westmount district of Montreal. We refer to these as 'Westmount dogs'. The experimental animals were raised in the laboratory, and they were raised in very small boxes. These boxes could be lighted artificially but they were lighted only on alternate days. Each animal was brought up singly, in one little box. These animals had no contact with the laboratory staff; they were fed by a special arrangement. We called these dogs 'restricted dogs'.

After a differential treatment of this kind for about six to ten months, in different experiments, the control Westmount animals were brought back to the laboratory, and then both the experimental and control animals were given a number of tests. All the animals were tested for emotional behaviour, motivated behaviour, intelligence and so forth. The results were as follows:

1. There was no difference in health and vigour between the two groups of animals. The restricted animals were just as healthy, their appetites were normal, and they ate as much as the animals in the

control group. Now, this particular finding does not agree with the findings of Réné Spitz, who finds that human infants deprived of maternal care show various defects, and particularly loss of appetite.

MEAD:

These dogs were mechanically fed, were they not? You do not have a human being around who knows that he is neglecting them. With the Spitz babies, they were entirely cared for by people who knew that they did not have a mother, and knew they were being neglected.

BINDRA:

The human infants had been brought up by a mother and were used to this before there was a disruption in this relationship. This is probably quite a different thing from not developing that relationship at all. I think some of the studies that I am going to mention later point in the same direction: what is injurious to the health of the infant is not the lack of mother-love as such, but rather the relationship being formed first, and then broken. This should be an important point in discussions of mental health because, if the rate of break-up in families continues to increase, it might be better to bring up children without strong attachment to one person only.

I think our studies show that it is possible to bring up a dog in a physically healthy condition without any care from the mother after four weeks, and without any close attachment to the mother.

FREMONT-SMITH:

Were there other dogs in cages near your restricted ones?

BINDRA:

I believe they could hear the other dogs.

FREMONT-SMITH:

They were not in separate rooms? They could smell each other and hear each other, but not see each other?

BINDRA:

That is right.

LORENZ:

May I make an objection? If a dog is isolated from other dogs only visually, while still being able to hear and smell them, I do not

think he would mind this at all. He would not feel lonesome in the least. So I cannot really regard your dogs as being socially isolated. If you had put them in sound-proof glass cases where they could have seen other dogs they would have been far more socially isolated. When did you separate your dogs?

BINDRA:

At four weeks.

LORENZ:

They are still nursing at eight weeks if you leave them. Until four weeks of age they were in the litter with a mother?

BINDRA:

Yes.

FREMONT-SMITH:

And they saw human beings, too?

BINDRA:

Yes, they probably saw their attendant.

TANNER:

Were they exercised when they were in the boxes?

BINDRA:

No.

LORENZ:

By four weeks a dog might know his mother personally. At four weeks he is able to run, he can gallop awkwardly, he will go urinating outside of the nest box and he will begin to eat.

BOWLBY:

For the first ten to fourteen days I understand it is very difficult to get the dog to learn anything at all (SCOTT *et al.*, 1951); but four weeks is long after that, and I wonder what information you have on their behaviour at that time?

BINDRA:

I have no details on that. I only have details on what happened when they first came out of the boxes.

BOWLBY:

Not when they first went in?

BINDRA:

Not when they first went into this environment. Since there is no mention of this in these papers by Thompson and Heron I believe they did not notice anything particularly worth describing. That is the best answer I can give you.

BOWLBY:

But if Lorenz is right surely they would have howled.

LORENZ:

You can assume that they would have howled for a day or so.

GREY WALTER:

The dogs who were taken away as pets may also have whined.

ZAZZO:

To keep our ideas straight, can you say to what age in a child four weeks in a dog corresponds; that is, in relation to the length of life and of the growing period.

RÉMOND:

It is about a twelfth of the way through the growing period. The dog's locomotion at four weeks corresponds to a child's at one and a half years.

ZAZZO:

In that case the separation was much later than that observed by Spitz, which was at six months.

LORENZ:

I think if we take a point of comparison, we ought to take the development of locomotion and of sense organs. A child of six

months is in a stage corresponding to that of a dog just opening its eyes. Very roughly speaking—you can only compare roughly—that would correspond to a dog ten days old which is just beginning to creep and to look about.

BINDRA:

There probably is an important species difference here. Given the same time of nursing, or rather the same proportion of time, the attachment in the human child may be much stronger, because the female dog nurses five or six animals at one time while the human mothers tend to spoil their infants.

LORENZ:

A child at six months will notice if a stranger bends over its crib.

BOWLBY:

Well, that is marginal; not all do till about eight or nine months.

LORENZ:

Anyway, if you transfer them at six months from their mother to a stranger, at first they will be sad and very easily cross, whereas I have once transferred a young dingo, a wild Australian dog, at roughly the corresponding stage to a foster mother. It did not make any difference to the puppy who was just about to open his eyes. He went for the foster mother as directly as for his real mother.

BOWLBY:

That would be in keeping with Scott's findings. After about ten days dogs begin to learn, but not before.

LORENZ:

I agree.

BINDRA:

Another factor that might be relevant here is that, in the case of these dogs, the separation from their normal environment is abrupt and continued for a long time, six to ten months, and this does not usually happen in the case of humans.

2. The next result has to do with *general activity* of the dogs, as measured in two tests. One test involved a simple situation, just a

room with one chair on which the experimenter sat. Each dog was brought in individually, and the experimenter noticed what it did during a period of fifteen minutes. A simple method of scoring the animal's activity was used. The second test of general activity was similar, except that it involved a more complex situation.

In the simple situation there was a great difference in the level of general activity between the restricted dogs and the control, Westmount, dogs. The restricted dogs were highly active, and they remained so for almost the whole duration of the testing period. The Westmount dogs, by contrast, were blasé; they just went in and sat down and did not engage in much activity. In the more complex situation, the difference between the two types of dog disappeared, that is to say, the free-environment or Westmount dogs began to show the same level of general activity as the restricted dogs.

A finding relevant to this is that younger animals in general are more active than older animals. So what seems to be happening is that the adult restricted-environment dogs behave like younger normal dogs, or (which probably amounts to the same thing) like older normal dogs put in a very stimulating (complex) situation.

TANNER:

NISSEN and RIESEN (1949) brought up some chimpanzees in restricted situations; one visually restricted, that is in darkness, and one with locomotor restriction, with limbs encased in heavy cardboard tubes. The net result was that they were retarded in growth, according to ossification standards.

BINDRA:

That sort of thing may be a factor in this experiment.

TANNER:

It might account for your saying that the restricted ones appeared younger.

LORENZ:

This can easily be answered by the weight; I expect the restricted chimpanzees were much lighter.

TANNER:

That is true.

LORENZ:

But these dogs were just as vigorous?

TANNER:

And weighed the same?

BINDRA:

Yes, there were no differences in weight; and actually one of the restricted dogs won a ribbon in a local show.

These differences between restricted and free-environment dogs have been followed for a year and last at least that long. So it seems that restriction early on in life does have some more or less permanent effect on their general activity. I doubt if the restricted animals will ever get as blasé as the free-environment ones.

3. *Problem solving capacity.* The animals were given four tests of ability to solve problems, and here we found marked differences between the two groups. The restricted animals did very badly in all problems, and this marked inferiority in problem solving has also lasted for over a year. I think this handicap will probably continue.

Now, some of our studies on rats (HYMOVITCH, 1952; LANSDELL, 1953) show very clearly that perceptual deprivation early in life has a more severe effect than the same amount of deprivation given later in life. I believe the same sort of thing might be true of our dogs: had they been given these six months of restriction when they were two years old they probably would not have shown so much deterioration in problem-solving capacity.

GREY WALTER:

The Westmount dogs had an extra six months of 'learning to learn', whereas the restricted ones did not have that opportunity. Naturally if you apply the same restriction later in life they would already have acquired the experience of learning to learn. If you have already had some practice at learning you have got that for ever, and no one can deprive you of it.

BINDRA:

Quite true, but, as I said, these animals remain inferior for at least a year after restoration to a normal free environment (in the laboratory), so they had sufficient time to get the same sort of experience. (One of our earliest experiments showed that bringing up dogs in the laboratory as against bringing them up in Westmount did not make any difference.)

GREY WALTER:

This is unlike the rather ill-controlled human experiments, where restriction of the opportunity to learn to read does not seem to affect very much the final level of reading ability; such children may be retarded up to a certain point, but they will come up to the normal curve quite rapidly.

BINDRA:

4. The next result has to do with the genesis of emotional behaviour. All animals were tested three to five weeks after the end of the period of restriction, and again eight to ten months later. The tests consisted in exposure to a number of stimuli designed to provoke emotional behaviour. Their behaviour was classified in three categories: general excitement, avoidance behaviour, and aggressive behaviour. In the first test, exposure to such things as an umbrella or a mask, or a little toy frog hopping about, free-environment animals showed in general avoidance responses. The restricted-environment animals, on the other hand, did not show avoidance responses but mostly general excitement, with no directed responses, either of avoidance or attack.

Eight to ten months later, on the second test, that is, when these restricted animals had had sufficient freedom and experience in the laboratory, they were given the same test again. Now the restricted animals showed avoidance responses and not general excitement, while the free-environment animals gave a high proportion of aggressive responses. I think this tends to show that these integrated patterns of emotional behaviour, withdrawal and attack, are learned, and not simply the result of maturation processes. The two groups of animals were of the same age but their behaviour was quite different.

LORENZ:

May I ask one thing? Your restricted dogs were, roughly speaking, retarded by about one year; they developed later with everything. Now your normal dog did not become aggressive against the frog and the mask and so on until he was about two years old?

BINDRA:

That is right, about two years old.

LORENZ:

A dog at one year is still rather timid. He will not become aggressive until two years. But I would believe that these things are not

innate only if your restricted dog did not show them at all. I would expect the restricted dogs to get aggressive just as much later as corresponds to their period of being restricted. Did this happen?

BINDRA:

In the restricted animals, we did not observe any aggressive responses on the second test. But I think the data on avoidance are crucial: the restricted showed avoidance on the second test, just as the free-environment dogs had on the first test, but the restricted animals did not show avoidance on the first test.

TANNER:

Did puberty occur at the same time in both groups? That is the time when any maturational effect is finally established and it occurs normally at about two years in the dog.

BINDRA:

I am sorry to say I do not know.

TANNER:

I should have thought that something like the times of closure of the secondary centres of ossification for the two groups of dogs would give you the answer to the question that Lorenz is raising.

BINDRA:

My feeling is that experience is the important variable in the development of these patterns of behaviour, and the general excitement in the first test is, you might say, the immediate response of the animal to a new situation. There are some studies on rats that show the same thing. For instance, if you give some sort of a painful stimulation to the rat it really does not *avoid* it, it just shows general excitement. If you observe the animal again and again in the same painful situation you find that eventually he develops an integrated pattern of withdrawal. In one experiment, we put rats on a highly heated platform, and all the rats did, on the first one or two trials, was to sit there. They were apt to pick up a paw and lick it and put it down, and pick up another paw, lick it and put it down; but they did not show any directed patterns that would get them away from the situation. Only eventually did directed withdrawal or avoidance responses emerge.

These findings also throw some light on so-called 'spontaneous fears'. Most normal adult animals show avoidance of a number of

stimuli. They avoid a snake, the cast of a snake if 'moving', and other such things. The question that arises about this spontaneous fear is 'Is it innate?'. Animals show avoidance of these objects even when they have had no previous experience with them. The chimpanzee will avoid a snake even when he has never had any painful experience connected with a snake, or even seen a snake (as you can know for sure in laboratory-bred animals). Quite obviously no specific learning has taken place; nevertheless I do not think the avoidance of a snake is innate. Even though no animal has specifically learned to be afraid of these objects, yet some sort of general learning, or general perceptual experience, is necessary before the animal will show spontaneous avoidance behaviour.

My argument is that avoidance is not associated with any particular object, but with strange objects in general. These animals have never seen anything like a snake. Most of their experience has been with things like bricks, with bars, with human beings and other things. So the first time you show them something different, they show general excitement, and, if they have already acquired some directed pattern, such as avoidance or attack, manifest that. Had these animals been brought up with snakes right from birth and not seen humans or dogs they would have shown fear, not of snakes, but of these other animals.

HARGREAVES:

If I remember rightly the fear of snakes appears to mature at a comparatively late age and that, before that stage, chimpanzees do not bother about snakes. This is rather like Lorenz's account of the I.R.M. to the hawk.

LORENZ:

I think that Prechtel has excluded the possibility that the reaction to the snake is simply the reaction to anything unknown. If you show them a pipe, which they have never seen, or a tortoise, they do not react at all, but if you show them a snake they make the typical gesture of disgust and wipe their feet on the ground, and that is very specific.

MEAD:

MASSEMAN and PECHTEL (1953) have tried out on monkeys a whole series of model spiders and scorpions and so forth that are much more related to a snake than a pipe or even a tortoise. They got the specific response only to the snake; the monkeys just ignored all the other livestock.

TANNER:

At what age does the reaction occur?

LORENZ:

With a child it is about two-and-a-half or three.

TANNER:

In a chimpanzee is it the same?

LORENZ:

If you raise a child in an environment of snakes, then you will find you can extinguish this reaction as you can that to every I.R.M. by what is called habituation. One of our Zoo directors made similar snake experiments with his own children and they don't mind snakes at all, because they had lived with snakes all their lives; while the hospital children of PRECHTL (1949) react exactly like the chimpanzees; specifically to snakes.

BINDRA:

Let me give you another example to illustrate the point with chimpanzees. The first time they were shown a part of another chimpanzee, the head actually, they showed avoidance responses. Now the interpretation I am suggesting is this: these animals have had experience of chimpanzees all along, but they had never seen the head of a chimpanzee alone. In order for this avoidance of the head of the chimpanzee to occur on the first presentation even though they had no previous specific learning, these animals had to have had previously the experience of a chimpanzee as a whole.

I think that this was also the case with those chimpanzees reared in darkness. They did not start showing fear of strangers or fear of other things immediately after they could see; they started showing this only after they had learned what their normal surroundings looked like. And then, when they were brought an unfamiliar object, they avoided it.

Fear of strangers, I think, also falls into the same category. If a child has been brought up with only one person, or two persons who have always taken care of it, and then exposed to a stranger, the child will show avoidance or general excitement.

MEAD:

You get it even in the middle of a native village where they have been brought up with thirty or forty people around them all the

time. There is still a point in maturation where they begin to discriminate and show fear of the people they see least often. The stranger can be someone who lives four houses down. It is very sudden, around eight to nine months as an average. But the people usually recognize it themselves, and so a baby that a week before was accepting any one of twenty people perfectly easily will suddenly develop all sorts of capricious fears and fright of the less familiar. For example, I would become immediately a 'less familiar', even with a child that previously would let me carry it. But if I had been taking a lot of care of a child, then I would be kept in the 'more familiar' class, although the ability to differentiate me when I am working with dark people is very good.

LIDDELL:

I think it is fair to say that there can be individual differences. There will be less of a response to a stranger by a child who has been used to many people than by a child who only knows one person.

MEAD:

There are individual differences among children, but I do not see any difference between societies where children are taken care of very generally and societies where their own mothers are prominent among several caretakers. But in those societies where the child is taken care of only by its own mother, it will show more fear, because in many instances such a child does not like other people.

BINDRA:

This raises the question whether loss of maternal care as such is the significant variable, or whether here again it is the attachment first having been made and then broken. I think our dog studies suggest that it may be possible to rear a child emotionally normally without a close attachment to one person.

MEAD:

But have you any experiment that bears on capacity for attachment? It seems to me that all your tests deal with avoidance and aggression. What do you have about the capacity to make friends and attach yourself?

BINDRA:

We do not have any direct evidence on this.

FREMONT-SMITH:

How about the dogs? Do you know whether they made good pets afterwards and became attached to one person in the same way as the ones that had not been separated at all?

BINDRA:

I should suspect not.

LORENZ:

I should expect the opposite. When you buy a dog which has been brought up under hospital conditions you will find that he is still free to attach himself. When a dog which has once attached himself is torn away—especially in the Lupus strains—he is more or less spoiled for ever. But I should be very surprised if your restricted dogs had lost the capacity of attachment.

MEAD:

Would you presume that a dog that had been hospitalized would attach itself in a normal way to one person, or might it have a variety of peculiar forms of attachment? Would it be as loyal, or too loyal?

LORENZ:

I should say it would be unselective and promiscuous in its loves for a time.

FREMONT-SMITH:

This would fit exactly with the concept of the psychopathic child.

LORENZ:

In different types of children, Sylvia Klimpfinger says, quite different reactions may take place. The children may become very introverted and show no tendency to show social attachment at all. On the other hand they may be 'all over you', show a lack of discrimination and be totally promiscuous in their attachments, and both may be the case with dogs, of course.

BINDRA:

That would suggest that the crucial variable is something quite different; it is not absence of attachment as such.

BOWLBY:

I think what is disappointing is that so far this experiment has not studied the functions in which we are interested. What psycho-analysis and mental health are concerned with are social relationships.

BINDRA:

Does this study not bear, at least to some extent, on the capacity to make relationships in humans? From the mental health point of view what is important is the sort of group a person is going to be making relationships in. If he is brought up in close attachment to one person, and later on finds that there is no such thing as an attachment to one person in the society where he is forced to live, then, from a mental health point of view, he would have been better off without that close attachment in his childhood.

BOWLBY:

I think we lack the evidence on which to make such a generalization, so that I rather deprecate the making of it.

BINDRA:

My point is simply this: your studies have brought up some very important variables that obviously affect the behaviour of human infants. I am interested in proposing some sort of a general theory that would account not only for your results but also ours. I visualize such a theory as having something to do with the number and kind of attachments the child makes as an infant. If he is brought up to make a close attachment to one mother you will get one type of result; if he is brought up to make attachments to a large number you will get another type of result, and neither is good or bad for mental health in itself. Whether it is good or bad would depend on the kind of society in which this person is going to live.

BOWLBY:

I think we are still very ignorant of what effect on mental health these different things have. It seems to me we know nothing from these experiments as to what effect on the dog's social relations or mental health the experience has—the evidence is not reported so we cannot draw any conclusions from it. The same is partly true of children where we only know a few things about certain extreme cases. What we know suggests that if a child is subjected to a

sufficiently large and changing population of mother figures, so far from being good at getting on with everyone he will be bad at getting on with anyone.

BINDRA :

When they are brought up in a particular society?

TANNER :

Does the cultural evidence support this?

MEAD :

If a child is subjected to a sequence of mother figures which do not recur or which recur at too wide intervals to be significant, then you will have instability. This is not the sort of thing which is described for primitive societies. What we have is a situation where, for instance, the child may be cared for by twenty people, but those twenty people are quite constant. The grandmother and mother and mother's four sisters and father's brothers' four wives and the seven little girls who live next door will all look after the child almost every day. So you have a constancy which is nevertheless subdivided among a number of people.

I would say that the cultural evidence suggests that on the whole there is an easier adult adjustment in these individuals than in the individuals who are cared for by single persons.

HARGREAVES :

It is important to draw a distinction between what you might call 'mothers in parallel' as opposed to 'mothers in series'.

BOWLBY :

Mothers 'in parallel' are probably all right—mothers 'in series' certainly are not.

LORENZ :

I have three or four different explanations for the result of your experiment. Your restricted dogs (*a*) may have missed some sensitive period with which they cannot catch up, (*b*) may have a slight detriment done to some I.R.M., (*c*) may be retarded in their maturation, and (*d*) may not be changed at all, but, as they have such very restricted individual experiences, they are from their point of view

in an entirely new environment. The normal dogs, on the other hand, having lots of experience of very varying surroundings, are always blasé to their environment.

This may also account for the lack of aggressiveness, because in a new environment one is not aggressive. A dog must feel territorial to get aggressive. So the environmental factor is able to account for this. But I would not dare to assert that aggressiveness is not innate. If you ask, 'Is suckling in a goat innate?' my answer is definitely that some of its components are and some are not. The rhythmical upwards-thrusting movement of the kid's head I would strongly suspect of being entirely innate, also, perhaps, a very rough orientation reaction which makes the kid search for the nipple 'on the underside of something'. On the other hand the 'knowledge' of the fact that the mother's udder is between her hind legs is quite evidently learned by trial and error. It is quite evident that there is no inborn orientation towards the udder by smell, as one might have expected. My point is that the mere question 'is aggressiveness innate' is much too simple in view of the possible complication of aggressiveness.

Another technical argument: if you take pure-bred dogs in order to get a more reliable genetic basis of behaviour, that is the worst thing you can do! So-called pure-bred dogs are always bred for form, and the more modern and popular pets they are, the less homozygotic they are as to behaviour. If a dog has the right kind of ears it may have monstrous anomalies of behaviour, yet dog breeders will still go on breeding from it. If you want a dog which is as homozygotic as possible as to behaviour, you must get a dog from a 'professional' police stud, or collies from Scotland, not the modern show-collies. My guess would be that you would get a more level curve of variability of behaviour even with mongrels than you would with high-bred dogs. As you know, the German shepherd dogs are already being bred in two strains, just as carrier pigeons are bred in different strains, and you find that those with a flat spectrum of performance are very poor in looks. If a policeman wants a good dog, he does not mind if his dog's tail curls a bit!

TANNER :

Are we not getting confused between two quite different genetical points? Inbreeding in the sense of brother/sister or father/daughter mating always tends to produce isogenicity and homozygosity for all genes, including those affecting both behaviour and form. We must distinguish this from selective breeding which is breeding for something completely different. If I remember rightly, you were saying that we should not use inbred dogs.

LORENZ:

No, I said pure-bred dogs, not inbred; severely inbred dogs would be all right. My tip would be to use dogs of a family which already has a characteristic 'spectrum' of behaviour as for instance dogs with comparatively high escape-reactions and at the same time high aggressive responses. It seems to me this very often happens in the same family. In Dobermans, it is very typical that you get in the same litter very aggressive dogs and very shy dogs, and other dogs which up to the third or fourth year are extremely shy and then get extremely aggressive. My tip is that these rather predictable types of extreme shyness or extreme aggressiveness would be more favourable objects for your purpose because they provide closer analogy.

BINDRA:

I think this is a very good suggestion. Of course, what we do will depend on the problem at hand. If we are interested in dogs of different responsiveness in order to see what is their susceptibility to neurosis, then we would have to follow something like you suggest. But if our problem is to determine the source of differences in responsiveness, then we have to use inbred dogs.

GREY WALTER:

I should like to make a mild and friendly protest. Animal breeding is a great art which has been cultivated over many generations, but not a single genetic proposition about the human being can be sustained at the present time; almost nothing is known about human genetics. I think people may have felt that there was some sort of hope of working out some of the paradoxes and dilemmas in human beings by some genetic trick or observation. I think that is a very faint hope and that even if you found absolutely clear-cut evidence of genetic traits in animals, we should still be quite in the dark as to the psychobiological development of the human child. We cannot envisage yet an experiment which will enable us to separate out the genetic and environmental factors in the behaviour of a human being. The application of animal breeding principles is a very specialized thing which has no relation that we know of to human beings.

LORENZ:

But Dr. Bindra wanted to investigate environmental factors; he was not interested in my genetically different dogs because he wanted to know what to do to prevent the development of anxiety by environmental factors.

GREY WALTER:

He is using animals which have been selected for many generations by breeders with a clear idea of what they wanted; human beings have not been through this process, as far as we know

LORENZ:

It is my opinion that they have.

GREY WALTER:

It is a thing that is impossible to prove and the evidence will still be only a dream as far as the human is concerned.

BINDRA:

That is true enough, but if we are to find some effects of variation in early environment we have to use inbred animals. We cannot generalize from that to humans, as you have said, but indirectly we derive some benefits from these animal studies.

FREMONT-SMITH:

May I ask for clarification? Dr. Grey Walter, you said that you thought there was no evidence that human beings had been through certain processes and dogs had been. Dr. Lorenz said he thought humans had. I would like to get on the record what was the process that you felt the dogs had been through although there was no proof that humans had been through it.

GREY WALTER:

The processes of inbreeding in the first place and selection for certain features, which we identify in cattle as milk yield, in dogs as the shape of the ears, or in cats as the colour of the coat.

FREMONT-SMITH:

You are making a distinction between a deliberate selection and a natural selection?

GREY WALTER:

Human beings have not been through a process where people choose one another as mates because they want people with red hair or they want people with high intelligence. That may have influenced certain cultures for a short time, but whether that has been true over many human generations cannot be proved.

MEAD :

In most small human communities, presumably the sort of communities which were the precursors of our large ones as far as we can tell, there is a very *high* degree of differential selection. In most societies where you have polygamy—and there are a very large number of them—the man who shows the traits that are particularly approved in that society can have five wives or twenty wives in the course of his lifetime, whereas the man who shows disapproved traits may be unable to get any wife, or only a woman who has proved herself infertile. The analogue to purposive planned breeding is very strong in small communities.

HARGREAVES :

I was thinking of a sentence of the report of the last session (Vol. 1) where you said that in some communities a woman with large breasts could not get married at all.

GREY WALTER :

On the basis simply of the mammary glands. A woman is selected for her mammary glands.

TANNER :

This is what you were denying before.

GREY WALTER :

There is some evidence from remote cultures, but can anyone lay down the principles on which mate selection has been based in Western cultures?

HARGREAVES :

From the sales of lipstick, red lips seem to be one thing. The sale of lipstick may have prevented that selection taking place, but it proves that red lips are a selection factor, otherwise you could not sell lipstick.

GREY WALTER :

With regard to the lips and mammary glands, in our culture we have produced a standard of lip redness and mammary gland development which is completely illusory; in this way we avoid selection of the characters we admire, by providing cosmetics and prosthetic appliances.

TANNER:

There is one matter where we have clear evidence of mate selection, and that is for physical size. The correlation of stature of spouses is about 0.2 in England.

HARGREAVES:

And intelligence is a selection factor as well; there is a high correlation in intelligence between married partners in our society. CAROTHERS (1953) maintains that does not exist in tribal Africa; equality of intelligence is apparently sought in marriage in our society and not in some others.

LORENZ:

There is another point of similarity between the breeding conditions of man and domestic animals, and that is the lack of natural selection. A wild animal species is continually subjected to a very severe selection: a wild animal is so cleverly adapted in shape that every tiny detail is carefully 'thought out'.

GREY WALTER:

By whom?

LORENZ:

By natural selection. You can change hardly anything; if you make a beak five millimetres longer or shorter it is evidently bad, and if you make the plumage of the female more roughly spotted it is apparently also bad. The right pattern may be determined in genetically different ways. MAYR (1942) has shown quite conclusively that wild species often are much more constant in their phenotype than they are in their genotype. This is the most convincing proof how sharp selection is! The main point of similarity between domestic animals and man is the complete removal of this effect of natural selection. Natural selection is much more strictly standardizing than that of the most exacting dog breeder. It would seem that this dropping out of the sharpness of natural selection is in itself sufficient to cause what we call domestication. Professor HERRE (1943) who studied the domestication of the reindeer by the Lapps, discovered that practically no factor in the life of the reindeer is changed as compared with the North-American wild caribou. The Lapps are actually unable to influence the life of their reindeer very deeply, because a Lapp family needs for its support more reindeer than it can hold. They have to run after the reindeer all the time;

they cannot keep them from moving. Only in winter a number of families congregate to drive the reindeer to certain islands in lakes where they can better ward off the wolves. So these reindeer graze naturally and have exactly the same ecology as wild reindeer. The only appreciable changes concern natural selection: the Lapps keep off the wolves and castrate the old bulls who get nasty. The domestication of the reindeer is comparatively young historically, and yet the reindeer shows every single trait of what Margaret Mead and I have agreed to call 'vulgarization' (see Vol. 1).

That is one factor which is really common between man and domestic animals. The other thing is what Julian Huxley calls reticulate evolution. The permanent process of branching up into races and into species, which is typical in the phylogeny of most animals, is prevented in man and domestic animals by their recrossing all the time. These are two extremely far-reaching biological factors which you find exclusively in man and domestic animals. And these are, after all, genetical things.

INTERVAL

BINDRA:

The second line of study that I should like to report deals with the effects of sensory or perceptual deprivation in the human adult. A considerable amount of recent neurophysiological evidence has shown that in order for the brain to function normally it has to be constantly bombarded by sensory stimulation. Sensory stimulation appears to have two separate types of function. Firstly, it evokes some sort of specific response (perception). Secondly it has a more general effect of arousal.

We were interested in finding out what the effects of sensory deprivation would be on a human adult. The method was quite simple. College students in need of money were paid twenty dollars a day to lie down in a specially prepared cubicle, with frosted glasses on their eyes. These frosted glasses prevented any pattern vision, though light went through. On their arms the subjects wore long cardboard boxes which prevented them from feeling any part of their body with their hands, though it was possible for them to bend their arms at the elbows. They communicated with the experimenter through a speaker system. They all contracted to lie there for as long as they could, and since the reward was twenty dollars a day, quite a few people stayed as long as four or five days. All the results that I am going to report are on subjects who stayed in at least forty-eight hours.

MEAD:

Do they contract to stay awake?

BINDRA:

No. They spend quite a lot of their time sleeping. The procedure for each subject was as follows: A few days before the subject was put in this chamber, he was given a number of tests. Some of these were sub-tests taken from tests of intelligence, and other tests were made up specially for our purpose. Then, during his stay in the chamber, the subject was given the same or comparable tests. He was given problems to do in his head. Immediately after coming out of the chamber he was tested again. We had a control group which did not go through the chamber but who were given the tests at the same intervals. The effects I am going to report have been corrected on the basis of the performance of the control group.

First of all, there was an impairment in intellectual function. About twenty-four hours after getting into the chamber, subjects began to report that they were unable to concentrate. They could not keep the questions in their head.

FREMONT-SMITH:

This is while they are in the chamber?

BINDRA:

Yes.

BOWLBY:

What are the instructions with regard to the voice? They might keep up a long conversation.

BINDRA:

Sometimes they did. There was an experimenter within reach of the subject all the time, not in the chamber but close by. He was always available because a subject might say, at four o'clock in the morning for example, 'I want some dinner' or, at one o'clock in the afternoon he might want some breakfast.

MEAD:

He is allowed to have food whenever he likes?

BINDRA:

Whenever he likes. Every time he asks for food, the experimenter asks him what food he would like. In that way we avoided giving any indication of the time of day.

MEAD:

No wonder he stays there and his intellect deteriorates!

FREMONT-SMITH:

Do they have any whisky?

BINDRA:

We have not yet been able to get any funds for that!

STRUTHERS:

I think I might bring in something about the conditioning which these men undergo. They frequently go on a binge before they start this performance . . .

BINDRA:

Soon after the subjects reported difficulty in concentration, they also began to show deterioration in objective tests. The error score increased.

TANNER:

Do you pay them to get the correct results? Because after a while you may get the 'couldn't care less' attitude—perhaps about the time they are going to give up.

BINDRA:

They seemed to try hard. They were quite bored inside the chamber because they had nothing to do. I think that that was enough motivation for them to try, and quite often they actually asked for problems.

They also reported some confusion. This confusion and disturbance was reported particularly when they had just left the chamber. Everything appeared different. The colours appeared to be too strong and so forth.

STRUTHERS:

The lighting is kept constant?

BINDRA:

Yes. The most dramatic effect they showed was some sort of hallucination—I think that is the only word to describe it, even though the subjects were quite aware that what they saw was not actually there.

BOWLBY:

The hallucinations take place with insight, and the patients know they are hallucinating?

BINDRA:

Yes. These hallucinations started off with relatively simple geometrical figures; the subject saw lines and triangles and things of that kind, and gradually they became more and more complicated. In the fully developed form they looked rather like Disney cartoons, that is to say, they had the quality of being fairly vivid and quite complicated. A subject saw a street corner, for instance, or he saw himself going out in a boat in Nova Scotia. As these hallucinations became more complicated colour appeared. The subjects were not alarmed by them so far as we can tell—in the beginning they were amused. Later they reported that the hallucinations interfered with their problem solving. One subject said, 'I am trying to work out this problem, but I still keep on seeing that damn street corner'.

FREMONT-SMITH:

If a subject has an hallucination, does he keep the same one or does he have a series of images?

BINDRA:

He has changes quite often, but he might report, 'Now that thing's back again'.

FREMONT-SMITH:

Did almost all of them have hallucinations or were hallucinations rather rare?

BINDRA:

Our first ten subjects or so did not report any hallucinations. But when one of the experimenters himself served as a subject and saw the queer things, he reported it, of course, and from that time on all the subjects were instructed to report anything they saw, or heard,

or if they had any unusual experience. After that, almost invariably every subject reported hallucinations coming on from twenty-four to forty-eight hours after he began the experiment. Presumably, before this instruction, the subjects thought there was something abnormal about seeing these things and therefore did not mention them.

MEAD :

Or you established a set.

BINDRA :

We took every care not to create a set. We only said in a general sort of way that if they saw anything they should report it.

MEAD :

'If you see anything' is an instruction, after all. How do you discriminate among types of eidetic images?

BINDRA :

These vivid hallucinations were not something they could call and let go at will. They just came.

FREMONT-SMITH :

Do subjects talk with one another in advance on the experiment? In other words, might one subject know what the other subject had experienced before he went in? Are they students?

BINDRA :

Students, yes, but students from different faculties. It is possible that some of them communicated with each other. I think the way these hallucinations appear suggests it is a genuine phenomenon. They appear more or less always in the same way: they start with simple forms and then get complicated. Even if the subjects talked about seeing things, I doubt if they would go into details of the manner in which the hallucinations occurred.

RÉMOND :

What is the importance of memory in your hallucinations, and also, how do you compare them with that sort of dream you may have while you are awake, particularly when you are in a state of mental fatigue?

BINDRA:

I think they are probably very similar.

WHITING:

May I ask if you expected your controls to watch out for certain things?

MEAD:

Who are the controls?

BINDRA:

Their chamber consists of a partitioned section of a room with a bed, and they have to lie down all the time, except when they want to use the toilet, or when they want to eat. They have no glasses or cardboard boxing.

FREMONT-SMITH:

Have they had any hallucinations?

BINDRA:

No, as far as I know nobody has reported any.

MEAD:

If you investigated subjects taking metabolism tests, you might find a large number of hallucinations.

BINDRA:

There are certain other instances of hallucinations. I am told that in many patients who suffer from migraine visual hallucinations are a common symptom.

GREY WALTER:

The teichopsia or fortification figures of migraine have a very characteristic crenellated form, but I do not see any relation between these and the experiences of your subjects. Mescaline hallucinations are the ones which seem to resemble most closely the ones you describe; these are often geometric figures, triangles and more elaborate patterns which develop into fantastic images related to real experiences and are also associated with the paranoid feelings of the subjects. I feel the accounts of your subjects were very similar

to those from experiments in which I myself participated. This suggests that if you have an interruption of the sensory inflow, it can have the same hypnotic effect that you get with mescaline, which acts at the same level.

BINDRA:

Is the metabolism of mescaline well known?

GREY WALTER:

It is becoming known. It might be interesting to try the effect of sub-threshold doses of mescaline to see if you could summate the two effects, to accelerate the appearance of hallucinatory experiences in the same experimental conditions.

MONNIER:

Did they have tactile hallucinations?

BINDRA:

Not tactile hallucinations, if I recall correctly, but some movement hallucinations.

MONNIER:

You would have expected this because you repressed sensations by putting a protecting sheet around the arms.

BINDRA:

The subject still had tactile stimulation on other parts of the body.

BUCKLE:

Can you say anything about the different types of people who have different types of hallucinations? For instance, body-image hallucinations I think almost always occur in schizophrenic people; do the hallucinations show any relation to the Rorschach test performance?

BINDRA:

There is no indication of any relation between personality and these hallucinations, but then we have not looked for such a relation very carefully. These are normal subjects, and with the small range of variation in the group we will probably not find any relation.

MEAD:

How are you controlling the sleep situation? Your major assumption is that it is being awake and unstimulated that is significant, and not being asleep. It is the periods of wakefulness within this constricting chamber that induce this perceptual deprivation. These experiments are of people studied with periods of sleep, are they not? For instance, I was once on a 'plane that had been grounded without putting the passengers in an hotel—just pushed to the side of an airfield in Texas. Under these circumstances I slept for sixteen hours. The lights went off, you could not read, there was nothing to do, so one felt one might as well go to sleep. I think it is quite possible that at the end of those sixteen hours my performance would have deteriorated; unless you can control the amount you do not know what is the effect of the sleeping done in this period.

BINDRA:

That is quite true, but most of the sleep occurs on the first night and the first day, and these hallucinations do not usually appear until thirty-six or forty-eight hours.

MEAD:

How about the deterioration in performance?

BINDRA:

They show increase in errors about twelve to twenty-four hours after entry.

MEAD:

I take it you think the hallucinations are the more interesting aspect, and the deterioration of the performance tests less so?

BINDRA:

Hallucinations are the more dramatic and were less expected; they are not necessarily more significant.

FREMONT-SMITH:

How long did it take the subjects to recover altogether?

BINDRA:

After they came out of this situation some of the earlier subjects reported having difficulty in driving. They also had difficulty in

judging distance. Later, we told all the subjects that they were not to drive an automobile directly afterwards. But they were quite recovered within a day.

FREMONT-SMITH:

How many students have been studied already together?

BINDRA:

This report is based on about twenty subjects, and there is another group of twenty-five that has just been finished.

The study (BEXTON, HERON, SCOTT and HEBB, 1954) does seem to suggest that the normal working of the brain depends on this sort of sensory stimulation, and that sensory stimulation has two separate types of effects: that of arousal as well as the more specific role of sensory stimulation in perception.

RÉMOND:

Do the subjects tend to become more and more sleepy as the time goes by, or just neurotic?

BINDRA:

Not sleepy. As a matter of fact, the general sign which warns the experimenter that the subject is ready to leave is that he becomes very restless, he starts fidgeting, and things of that kind.

TANNER:

Does he hallucinate more and more?

BINDRA:

I have not had that reported.

MEAD:

Have we any reports on the behaviour of adults who have been subject to eye operations which were sufficiently acute that they had to remain in bed in a small hospital room, and the only sort of communication they had was with a nurse? I would suspect that you might get the same results. You should, unless the purposeless restriction is the thing that induces the hallucination.

RÉMOND :

I have a friend who has had that situation after a retinal detachment, and was obliged to stay for about a fortnight in complete darkness with a bandage over his eyes. He could not chew because of the operation they made on his eye, so he could only receive food by tubing. He was a doctor, a neuropsychiatrist, so he was quite aware of things in that field of experience; anyway, after three or four days, he became neurotic and threatened to break everything in the room. Then he stood up to the situation, but lost about 20 kilos of weight.

GREY WALTER :

But was there not some anxiety about the operation?

RÉMOND :

He knew he was probably going to lose his eye, and his other eye was already very bad.

MEAD :

I was thinking of the condition where the deprivation was purposeful. One of the elements in this experiment is that it is so purposeless, except for the twenty dollars motive, which is obscure.

WHITING :

I know someone who had an operation on her eyes, and she had quite a clear hallucination, she thought she was in another room. She was supposed to lie on her back, but she sat up in bed and said 'I am in the wrong room'. It was a very strong hallucination, and when the doctor came he said: 'This very often happens in cases like this; don't worry about it'.

MONNIER :

I would like to emphasize Dr. Bindra's statement that to understand the effects of perceptual deprivation we must keep in mind that sensory stimulation has two functions: a specific and a general function. I would even say that when you suppress the specific function of sensory stimuli there is a great chance that the unspecific function of this stimulation will be enhanced. We could also say that suppression of a special function in the nervous system enhances the other partial functions. In my experiments on coagulation of the thalamus in humans I observed a similar thing. If I coagulate the

thalamic centre of pain in the arm this part of the body becomes anaesthetic, but pain develops now two or three days later in the leg. This means you cannot suppress something specific in the nervous system, without the other surrounding systems becoming automatically enhanced in their function (MONNIER, 1953). It is possible therefore that in Dr. Bindra's investigations suppression of specific sensory impulses, such as optic or tactile ones, enhances the unspecific functions of the visual and tactile cortical centres and that hallucinations arise.

BINDRA:

The next line of research that I am going to talk about concerns individual differences in reaction to stress. Such individual differences have been noted again and again. In much of the work on experimental neurosis investigators have found that some animals break down and others do not. Even in ordinary laboratory tasks such as solving analogy problems one finds that the performance of subjects does not necessarily suffer on exposure to stress. Some individuals keep on working at the same level, some deteriorate, and some actually improve their performance. Obviously there is some variable of individual personality difference which determines which subjects are going to react in each of these ways.

A number of investigators (e.g. FARBER and SPENCE, 1953) recently have tried to suggest that the variable *anxiety* is the crucial one. The general idea is that the performance of people who are anxious will deteriorate under stress; people who are not so anxious will probably remain the same; and people who are not anxious at all might improve.

There have been about sixteen investigations in the last three years on the American continent made with this hypothesis in mind. The investigators have tried to define anxiety in a number of ways, some in terms of Rorschach indices, others of psychometric scales; others have taken ratings and the opinions of a number of clinical psychologists. Then they have exposed the subjects to stress and tried to determine its effects on high and low anxiety subjects. The results in investigations using Rorschach indices and psychometric scales have been negative, that is to say, no differential effect of stress was seen on subjects who had been differentiated on this basis. Nevertheless, some studies (e.g. EICHLER, 1951) do suggest that what clinicians call anxiety does have something to do with performance under stress. Whatever variable determines the responses of subjects to stress is probably related in some way to anxiety, though it is not anxiety as determined by the Rorschach indices.

LORENZ:

I quite agree. I think the Rorschach indices are at fault.

BINDRA:

The approach that we have taken on this problem is this: anxiety is too vague or general a concept, and if there is an individual difference or personality variable that is related to stress, it is probably only one *dimension* of what we vaguely call anxiety. For the present, especially in clinical situations, we have to go on using the concept of anxiety, but we should make an attempt to get some more precisely defined, unitary dimensions which would replace what is now called anxiety. When the physicists took over from the layman the concepts of hot and cold they could not do much with these common-sense ideas, and gradually they evolved more precise dimensions such as temperature, wind velocity, humidity, and so forth. Hot, warm and cold still remain useful concepts in everyday life, but in order to develop any exact studies one has to deal with these more precise dimensions. One of the theoretical aims of this research, therefore, was to analyse anxiety and to replace it by more precise dimensions.

FREMONT-SMITH:

After you have taken the cold day apart in terms of wind velocity and so on, you find you have lost your cold day.

BINDRA:

Of course, you could still use the 'cold day' for certain discussions.

FREMONT-SMITH:

You really have to blend all these units together again, and then it does not quite give you a cold day when you get through.

LORENZ:

That is quite typical of so-called 'injunctive' concepts and definitions. We think of most complex realities as sums of very many constituent characters, some of which may be missing without making the conception inapplicable to a given phenomenon. Therefore it is, on principle, impossible to give a definition of a man, or an animal, or of life itself. The term is, so to say, 'loaded' (*injunctive* means something like that) with a set of qualities, which form a sort of spectrum, grading to both sides into that of another

concept. It is a very fundamental error to think that a concept for which you cannot give an implicit definition is not a concept. There are hardly any biological concepts other than these. Some people always say, 'Well, define innate behaviour, and if you cannot, go home'. My answer to this is, 'Define man'.

BINDRA:

I think this greater precision is necessary only at certain times. We can carry on many discussions with *anxiety* and do useful things with it, but sometimes greater precision is necessary.

For various reasons, into which I shall not go just now, we thought that what we have called *responsiveness* is one such variable or dimension. It is probably related to what is clinically called anxiety, and at the same time we thought it might differentiate people with respect to their response to stress.

GREY WALTER:

When you say variable, do you mean an interpersonal or an intra-personal variable?

BINDRA:

An individual difference or personality variable, as opposed to what may be called an experimental variable: a property that, for the most part, does not vary within the individual, but varies from individual to individual.

LORENZ:

Why did you not simply call it readiness to anxiety?

BINDRA:

'Readiness to anxiety' would imply that this is an essential part of what is called anxiety. I do not think it is. I think there are many other things involved in 'anxiety', and responsiveness is only one aspect of it.

The measure of responsiveness is really quite simple. It is based on lack of discrimination between a threatening stimulus and a non-threatening stimulus. Two lights, *left* and *right*, are presented to the subject in a random fashion. One of the lights is always followed by an electric shock, and the other light is never followed by a shock. Galvanic skin response is recorded continuously. Each light is

presented twenty times. There is a very characteristic galvanic skin response when the subject is given a shock or when the shock-light appears. The subject gives a reaction when the threatening shock-light is presented, and then he gives a reaction to the shock itself. But many subjects give the same 'double' response to the other light as well, that is to say, to the non-threatening stimulus.

LIDDELL:

Does the shock always follow at a certain interval after the light?

BINDRA:

Yes, after a standard interval of about three seconds. We just count the number of the characteristic double responses given to the negative (non-threatening) stimulus. This number is taken as a measure of lack of discrimination, or responsiveness. We get a tremendous range of scores. There are subjects who do not respond to the non-threatening light at all, and others who respond to the non-threatening stimulus on almost all occasions. On the basis of these scores, we divided eighty subjects into three groups: high responsive, medium responsive, and low responsive.

GREY WALTER:

Are the number of anomalous responses distributed normally?

BINDRA:

For this group of eighty subjects they were, more or less.

These three groups did seem to respond differently to stress as determined by changes in their performance on certain tasks under stress. In general, we found that low responsive subjects actually improved their performance under mild stress. The stress in this particular experiment was the *threat* of an electric shock. The high responsive subjects deteriorated, and the medium responsive subjects did various things. The task they have to do is fairly complicated. To begin with, they learn Chinese symbols for different numerals.

GREY WALTER:

It is a sort of code?

BINDRA:

Yes.

GREY WALTER:

How far up did you start, then? You did not start them on two or three, did you?

BINDRA:

All the digits that were given to them in the performance tests were digits over 20.

FREMONT-SMITH:

When was this performance test done in relation with the right-hand and left-hand lights?

BINDRA:

The non-stress performance was done first, then came performance under stress, and then after an interval of about 10 or 15 minutes responsiveness was determined.

FREMONT-SMITH:

But the stress performance was done while they were being shocked?

BINDRA:

No, there was only the threat of shock. The subject is taken to a table where he is given an electric shock. Then he is told, 'This is the type of electric shock you are going to get. Sit down here, and put your left hand on this shock-grid'. There are all sorts of switches and gadgets to scare him. Then the experimenter tells him that while he is working on the task he will receive an electric shock on his left hand. But actually no shock is given during performance. Stress is simply *threat* of shock.

BOWLBY:

Irrespective of how he performs?

BINDRA:

Yes.

BOWLBY:

The promise of what he is to get bears no relation to how he performs? He is not told that if he performs well he will not get a shock?

BINDRA:

No, he thinks he is in for it anyway.

We compared the non-stress performance with the stress performance, and found significant differences between the three sub-groups, high, medium and low responsive. However, one complicating finding was that the effects of stress were not the same for different components of performance. If you take speed as the measure of performance, you get a different result from that obtained by scoring errors. The most significant differences between high, medium and low responsive subjects were differences in speed. The low responsive subjects, when they performed under conditions of stress, actually increased in speed. Medium responsive subjects did not increase in speed. The high responsive subjects did not increase in speed either, and they showed some disturbance in performance in other ways, either in errors or in the number of items attempted but not completed.

GREY WALTER:

In these problems, are they just deciphering a coded series, or are they doing sums?

BINDRA:

They are given the figure 80, say, in Chinese, which they translate into English.

GREY WALTER:

Do they have a list of the symbols at hand?

BINDRA:

No, they learn this beforehand. Say they are given the figure 80 in Chinese, they have to translate that into English, halve it, and then find the Chinese for 40.

GREY WALTER:

I do not know why on earth you chose this particular problem. It seems to me a most intractable situation. There are so many variables involved—skill at calligraphy, linguistic ability and so on.

BINDRA:

If the task is too simple the amount of stress to which you have to submit the subject in order to get any performance decrement is

very high, and people are not willing to be subjected to that amount of stress. If you want to reduce the intensity of stress you have to increase the complexity of the task.

GREY WALTER:

I agree, but I still doubt whether it should be so intricate.

BINDRA:

It should be a problem with which everyone has had the same experience, as far as possible. I should also say that there was an experimental group and a control group, and the control group went through essentially the same problems and the same procedure, except that subjects in the control group were not subjected to stress.

BOWLBY:

What sort of association was there, in fact, between the high responsive people and performance—what sort of correlations were there?

GREY WALTER:

What were the actual statistics?

BINDRA:

They were not treated in terms of correlations. They were treated in terms of significance of difference between the various sub-groups.

GREY WALTER:

't'-tests?

BINDRA:

Yes, 't'-tests. The high responsive group gave a significantly higher number of items uncompleted as compared with the low responsive ones.

GREY WALTER:

At what level?

BINDRA:

At about one per cent.

BUCKLÉ:

What kind of variability do you get by repeating the tests on the same subjects? Do you find the reactions reliable?

BINDRA:

We have split-half reliabilities on the responsiveness trials. If I remember correctly, there were about forty trials and we got a correlation of about + 0.8.

LIDDELL:

What is the total duration of the test?

BINDRA:

Three minutes.

LIDDELL:

Everything that you do to the subject takes how long?

BINDRA:

About an hour and 15 minutes.

MEAD:

I am worried about something a little different. You did not test responsiveness, in the sense of the confusion between the two lights, until after the subjects had undergone the stress performance?

BINDRA:

That is right.

MEAD:

In other words, everyone had learnt something about electric shocks combined with the demand to do something, and then you gave them this responsiveness test. Did you not control it by taking another group and giving them the responsiveness test first?

BINDRA:

No.

MEAD:

In other words, you taught them the piece of confusion that they were supposed to have.

BINDRA:

If you want to put it that way; but it did not confuse everyone equally.

MEAD:

But, you see, you have got a systematic relationship between the stimuli in the stress and in the responsiveness situations. If, instead of using the electric shock for your stress situation, you had a bull around the corner or a dog that was going to bite or something and then used the electric shock for the responsiveness situation, you would not have had this possibility.

My point is simply that what you test, among other things conceivably, is sensitivity to the idea of electric shock.

BINDRA:

Yes.

MEAD:

The stress performance is a sensitization to the threat of shock. That is a learning experience about shock, so all your subjects have gone through a learning experience to which they have responded differentially, for a reason that you do not know. Then you put them in another experience when they use that learning, and they again give you a measure of their sensitivity to electric shock—or fear of shock—whereas if you made those threats completely different I think you would have a better result.

Also, it would be useful if you could do a set of tests to show how much failure to discriminate stimuli these individuals show. You could have different stimuli with the same affective tone and then very similar stimuli but with a positive affective tone. You might be dealing with 'anxiety'—that is, with a tendency to over-generalize—from the threatening. But you might be dealing with a tendency to over-generalize on the basis of *any* affective tone, either threatening or pleasant, and that would change your theoretical result.

BINDRA:

Yes, this is a very good point.

GREY WALTER:

There is another problem in this experiment—the variation in the sensitivity to electric shock. There are many people I know—not necessarily horny-handed mechanics—who can stand a shock from 100 volts or so and not mind about it at all, and others whom six volts will hurt. It varies greatly in different people.

Was this a very supra-maximal shock—tens of thousands of volts or so—from a high resistance source?

BINDRA:

No, it was a mild shock.

GREY WALTER:

Your results may be diluted by this factor: some of the people who look as though they had a high responsiveness might in fact simply have a rather low threshold to electric stimulation.

BINDRA:

Yes; unfortunately we were unable to control this.

LIDDELL:

This factor is important; in a psychological laboratory where we were using simple conditioning with mild electric shock on the fingers we had to abandon it because girls were having hysterical crying fits for no reason.

GREY WALTER:

In my laboratory we have a lady who cannot stand a 12 volt D.C. shock but her husband does not worry much about 100 volts. It seems to be just a question of special skin quality.

BINDRA:

I think all you have said is quite true, and I think what we have called responsiveness is responsiveness measured in a particular type of situation. It is a matter of making this variable more precise gradually, by seeing exactly what it is that makes for the obtained individual differences.

Now we plan to do some experiments on dogs. We are going to see if the same sort of differences in responsiveness exist in dogs, and, if these differences in responsiveness do exist in them, we are going

to subject dogs to stress, and then determine whether or not responsiveness is related to susceptibility to neurotic breakdown.

I can now go on to describe another line of research at McGill. This study has to do with the reinforcement theory of learning. Ever since THORNDIKE (1928) put forward his law of effect, saying that connections in learning are strengthened by reward and weakened by punishment, psychologists have been trying to discover whether or not rewards and punishments are necessary for learning to occur. There have been two schools of thought, one saying that reward or punishment or some sort of reinforcement is necessary, and the other saying that reinforcement though very important is not absolutely necessary. Most of the experiments have made use of the learning situation of pressing a lever or running through a maze or some such thing, and then created a motivation in the animal, usually by depriving it of food or water. What exactly happens within an organism when it gets a reward (e.g. food or water) or a punishment? Of course, the food goes into its stomach but in order for this reward to leave any permanent learning effect it must have some effect on the brain. The question is what are the neurophysiological mechanisms involved in this reinforcement.

In the last few weeks two researchers in our laboratory, Olds and Milner, have made a frontal attack on this problem. They have demonstrated reinforcement in the rat by direct intracranial stimulation. They have implanted electrodes, we believe in the anterior group of nuclei of the hypothalamus. Electrical stimulation in this part of the hypothalamus has the same effect for all practical purposes as reinforcement by food in the hungry animal.

LORENZ:

Do you mean they make eating movements?

BINDRA:

No. Perhaps the best example to describe is the Skinner box experiment. A rat is placed in the Skinner box, which is an ordinary box with a lever or a bar sticking out on the inside. The animal is able to obtain a pellet of food by pressing the bar. To begin with it presses the bar accidentally, but gradually it learns that pressing the bar means food and then presses the bar, if hungry, at a very rapid rate.

Now, Olds and Milner do not deprive the rat of food or water; there is no question of any drive or motivation in the usual sense of the word. The animal is placed in the box and accidentally happens

to press the bar. When it presses the bar an electrical circuit is made and it delivers through the implanted electrodes a certain amount of current in that part of the brain. What happens then is that very soon the animal begins to press the bar again and again at a rapid rate. One gets almost the same learning curve as you obtain in a standard Skinner box experiment, involving food deprivation and food incentive.

FREMONT-SMITH:

Does the animal salivate?

BINDRA:

No. The interpretation I like is that there is something common to all sorts of reinforcement—when the hungry animal gets food, when the thirsty animal gets water, when the animal which is being shocked finds means to escape from the shock, etc. It is probably this something in common that is involved in the hypothalamic stimulation. This is only one possible interpretation, and it may be quite wrong.

HARGREAVES:

Does the animal become satiated?

BINDRA:

Yes, it does.

HARGREAVES:

I wanted to know whether he became an electroholic. Is it an appetite or an addiction? Is it an appetite that can be satisfied?

BINDRA:

It is closer to an addiction, though it can be satisfied. One other result. If you disconnect the circuit, the bar-pressing response can be extinguished. The animal presses the bar and, not receiving any stimulation, looks round as if to say to the experimenter, 'What did you do?'.

LORENZ:

He is disappointed?

GREY WALTER:

Roughly how often will he do that before extinction; twenty, one hundred times?

BINDRA:

About ten or fifteen times. Of course, with repeated reinforcement and extinction things change. One reinforcement trial will reinstate the response, and the rat will go on pressing like mad, and one or two non-reinforced trials only will be sufficient for extinction and will make him give up.

LORENZ:

It is, I suppose, quite irrelevant whether the rat actually chews and salivates, in other words whether his motor effectors perform the movement. He may be stimulated in such a way that he is 'eating in the higher centres' or 'believes that he is eating'.

GREY WALTER:

This is a very difficult problem because the result depends on where your electrodes are exactly. If they are in certain centres, then the whole brain will get information as if the animal had satisfied himself. But a movement of half a millimetre, probably, or a change of frequency of stimulation might have quite a different effect. Your colleagues have been very fortunate, or clever, in happening to get just the right region.

BINDRA:

They have been playing around with this sort of thing for about five years.

GREY WALTER:

There is a very serious danger that your laboratory will become a physiological laboratory!

LORENZ:

I think this is one of the major findings!

BINDRA:

I should like to get the feeling of this group regarding interpretation of this phenomenon.

TANNER:

When he is satisfied does he then eat after that, or does he think that he has eaten? That part of the hypothalamus is, in all probability, the centre which tells you whether you are hungry or not. This is a matter still of some disagreement but it may well be that you are doing exactly the same thing as increasing the glucose concentration in those nuclei. When he is satiated there, is he hungry or is he not?

BINDRA:

I do not know what he does when you put him back in his own cage, but it is my impression that the rat does not show any peculiarities of behaviour.

GREY WALTER:

Does he lose weight?

BINDRA:

No, I don't think so.

LORENZ:

I think this question can be answered by a rather simple experiment. If you deprive this rat of food, you can then show the equivalence of real eating and being stimulated by the electrodes.

BINDRA:

If you satiate an animal normally and put it in the Skinner box, it will not continue to press the bar—because it is satiated. In our experiment, we have a food-satiated animal, but it keeps on pressing the bar so long as it gets the current delivered.

FREMONT-SMITH:

Therefore he must be unsatiated in some respect.

BOWLBY:

Have you put electrodes in other parts of the brain?

BINDRA:

So far in only one rat. The results are not the same.

I think the crucial question here is whether or not we are stimulating

the place in the hypothalamus that is involved normally in reinforcement. The crucial experiment might be to give a very strong current in this part of the hypothalamus and destroy it, and then try to make the animal learn by ordinary hunger-food motivation to see if the animal can learn in the same way as normal animals.

LORENZ:

You could destroy it and then see whether you have destroyed enjoyment of eating.

FREMONT-SMITH:

Do you mean you would see if you had destroyed their learning centre?

BINDRA:

No, not the learning centre but the reinforcement centre, or the pleasure centre.

MONNIER:

My impression is that you *increase a readiness to activity* by stimulating this hypothalamic centre; this fact has been observed very often. According to my conception, the rat in your experiment presses the bar again and again, because you increased its readiness to activity by stimulating its hypothalamus, just as hunger, thirst or rage would do. In such states of increased readiness, an activity once started keeps going on.

BINDRA:

No. Activity effects are not cumulative, but we do observe cumulative effects in this experiment. The first time the animal learns it presses the bar and does not press it again for some time, and then gradually it improves until finally it reaches a fairly high steady rate. Now you stop reinforcing the response, and it is gradually extinguished. But the second or third time you start reinforcing the same animal it starts pressing straight away, and the response can be extinguished straight away too. If this phenomenon were due to increased activity brought about by hypothalamic stimulation, you would expect that the same amount of stimulation would continue to have the same effect every time. But we observe a cumulative learning effect.

MEAD:

There is something we do not know anything about, one of those common-sense notions which we call 'gusto'. Now gusto is associated with eating, drinking, copulating, and all these various pleasures. People who are good and hungry, or good and thirsty, will eat and drink with gusto. Gusto is a concept or a description which includes both appetitive behaviour and increased activity.

LIDDELL:

Zest.

MEAD:

Yes, zest. That might be an area worth exploring, and very possibly people learn better the things they do with gusto, even though you cannot attach either thirst or food or something of that sort to them. We know that the people who do anything with gusto and enjoy what they're doing, tend to do it well.

BINDRA:

These animals may act with gusto, though we have not noticed, but the question still remains why should the animal choose this particular response or act to perform?

MEAD:

I am suggesting that gusto is self-rewarding and reinforcing, and gusto may also be associated with increased activity as well as with satisfaction of appetite. That would be the bridge with what Dr. Monnier said.

TANNER:

If in a similar experiment one rewarded or gave gusto to the beast every time he *ate*, I wonder what would happen, would he just go on and on and blow up eventually?

HARGREAVES:

What about feeding him with some 'gusto' just when he is approaching to eat in order to see if you could short-circuit the eating?

BINDRA:

Another thing would be to make the animals actually hungry before putting them in the box to see if the learning curve becomes steeper.

GREY WALTER:

I do hope your colleagues will extend their experiments to other animals before they spend too much time and ingenuity, for the rat is a very difficult subject for neuroanatomists. It would be fascinating to see the results in the cat where these anatomical extensions have been worked out so carefully.

BINDRA:

We do not have any cats in our laboratories, and with dogs we have not had much success in getting electrodes implanted.

MONNIER:

Dogs are not very suitable, because of the extreme variability in the size of the skull.

BINDRA:

The hypothalamus of the dog has not been mapped out, has it?

GREY WALTER:

The cat has been charted with extreme care by Whitlock and many others.

BINDRA:

These comments will be very helpful to us in our further work.

THIRD DISCUSSION

Presentation: Dr. Liddell

LIDDELL:

It seems to me at this time that it would be appropriate to raise certain puzzling questions concerning the psychobiological development of the child. This area of difficulty is well stated in a recent paper by BOWLBY (1953) called: 'Some Pathological Processes set in train by Early Mother-Child Separation.' He says in conclusion:

'Such experimental work as exists suggests that responses learnt under stress are far more resistant to extinction than those learnt when the organism is relaxed, and that exposure to one stress situation has significant effects on later reaction to another by lowering the threshold of susceptibility.'

'Thus we find ourselves confronted with the laws governing initial learning by immature organisms in conditions of stress. Unfortunately the overwhelming majority of experiments in the field of learning study exactly the opposite state of affairs—the laws governing later learning in mature organisms not in conditions of stress. It is evident, therefore, that if learning theory is to help us with these central problems of psychopathology, it will need to be extended to cover the special conditions described. Such an extension is greatly to be desired, since it seems likely that it will establish the link between human psychopathology and studies of experimental neurosis.'

Since Darwin and Huxley placed man in nature 75 years ago, there he has remained. And those of us who are making common cause in the study of these fundamental problems of grievous mental injury in early youth must somehow come to a common basis of understanding as to methods and aims. So it seems to me that certain fundamental and puzzling questions that concern us all are the following. What is the nature of the so-called psychic trauma? What mechanisms can the neurophysiologist, the endocrinologist,

the experimental biologist, discover to account for the consequence of intolerable stress imposed on the immature organism, remaining as scars upon the personality or brain perhaps for the duration of life?

Secondly, how is it possible for the mother to shield her immature offspring effectively against these otherwise mentally injurious environmental stresses?

These problems concern those of us dealing with infra-human organisms, as well as those who, like Dr. Bowlby, work in the clinical field. It seems to me a fundamental point of methodology — and this to me is the significance of the use of the term psycho-biological—that we have not here a closeted self-limiting laboratory field of operations. So soon as we bring an organism into the laboratory and purport to control the conditions under which we impose stress upon it, we are of logical necessity forced to pursue that organism outside the bounds of the laboratory into its natural living quarters and free-roaming space.

So I thought I would begin by stating that from my point of view there are no commonplaces of behaviour, and any notion that there is routine, or usual, or unnoteworthy behaviour cannot possibly be true. Therefore in introducing problems of methodology I wish to give two or three examples of seemingly commonplace behaviour which would be incomprehensible provided one did not make a psychobiological survey of the animal's life situation.

The first example is this. My naturalist colleague Dr. Nicholas Collias was serving as midwife to a pregnant ewe who was about to lamb. He was out in the barnyard one afternoon when he saw this pregnant ewe staring intently at a pool of fluid. He rightly anticipated that she was about to give birth. Therefore, he stood by, he delivered the lamb, took it at once and prevented the mother from making any contact with it. She could only smell it from a short distance. He used this to induce a magnet reaction in which she faithfully followed him into the laboratory room where he incarcerated her, because another lamb was due. Then he took her firstborn to a distant room and separated it. He came back to assist the mother in her second birth. This twin he allowed her to deal with in the normal manner. She licked it, she induced it to nurse and so on. Four and a half hours later he returned the firstborn. The mother would have none of it; she was already in possession of her properly tended lamb. To permit this firstborn lamb to get nutriment, it was necessary to confine the mother to a stanchion so that the lamb could nurse in spite of her unwillingness to permit it to do so. Then Dr. Collias kept the mother, the wanted twin and the unwanted twin in the same experimental compartment. All of the compartments are fitted

with a one-way screen, so the experimenter can withdraw from the room to see what is happening. He watched the mother and timed her rejection butting movements against the unwanted lamb for an hour, and made a note of the number of these. Over the course of three and a half days, her rejection became less and less vehement. She butted the lamb fewer and fewer times, and finally it could occasionally get a drink, but it was permitted to nurse regularly only while the mother was confined to the stanchion. I have a small laboratory class which is conducted as follows. The students have no textbook, they simply see the animals and are taught to write down all that they observe. I brought the class of students into the room with this mother, the favourite twin and the unfavoured twin. I had not seen the mother with these twins before, nor had the class. Dr. Collias had told me privately what had occurred, and I said to the class, 'Now this mother has a favoured twin and an unfavoured twin; let us all guess which twin is which by observing their behaviour'. The mother stood across the room facing us, gazing at us intently. One lamb began wandering round the room, the other made directly for the mother's teat and began suckling. We all chose the wrong lamb! This mother was so vigilant and alert to our watching her that the unwanted lamb was able to slip in 'under the wire' and get a free meal. This is what I mean by saying there is no commonplace of behaviour.

The second example is this. The same class, within a week, was taken into one of our laboratory rooms, 10 feet square with a cement floor. We lined up against one wall. There was a lamb in this room three weeks of age. None of us had seen this lamb before. The lamb stood in the farthest corner, wheeled and faced us, ready, tense, and if one of us approached it would attempt to escape. After some minutes we filed out of this room and went single file into the adjoining room and lined up similarly against the wall. There was another three-weeks' lamb who was a twin to the first one. At once when we lined up against the wall the lamb dashed to us and tried to force itself between our legs and the wall to make as close a contact with us as possible. The difference in behaviour was easily explained. The second twin had been removed from the mother at birth and raised on a bottle by the laboratory staff, though we ourselves were strangers to it and it was a stranger to us. If Dr. Lorenz chooses to extend and expand a little his concept of imprinting, perhaps this is the mammalian homologue of imprinting, which will not last very long in the sheep.

A third example: one of our goats, trained for several months in the laboratory, was observed as it wandered about in the barnyard. A new electric fence had been installed and the goat shortly

approached this unfamiliar strand of wire. It hesitantly touched the wire with its muzzle and instantly wheeled and dashed away. But after a few steps it wheeled again, faced the electric wire and precisely flexed its right foreleg. The casual observer would be at a loss to account for this unfamiliar or peculiar instance of goat behaviour. However, the circumstances of the animal's training make its precise but bizarre reaction to the unexpected shock on its muzzle more understandable. It had been trained in the laboratory for several months according to the following regimen. For an hour each day, while confined by a restraining harness and with electrodes attached to the right foreleg, a buzzer was sounded for 10 seconds, followed immediately by a mild electric shock to the foreleg. Forty of these buzzer signals, spaced a minute apart, were given at each session. If the goat kept its foreleg flexed until the buzzer stopped it received no shock. It learned very soon to avoid the shocks and, indeed, had not received a single shock for several weeks. However, its prompt and precise flexion at the sound of the buzzer continued without lapse.

Now, in the barnyard, the novel experience of electric shock on the muzzle promptly released the inappropriate behaviour of running from the fence, wheeling and *then* making the avoidance response—a most unrealistic reaction to a situation meaning danger.

During the past two years further observations indicate that this goat has developed a definite and chronic emotional disorder (which is all that is meant by Pavlov's unhappy term 'experimental neurosis'). Except for the dramatic episode of its unrealistic reaction to danger just mentioned, a visitor unfamiliar with the history of this animal's behaviour would fail to note any striking peculiarities setting it apart from our other goats. However, we accidentally discovered that its pattern of emergency reaction to danger had become highly simplified and stereotyped. We no longer give shocks no matter what the animal does during the test period, and although the buzzer signal has now been repeated more than two thousand times at irregular intervals and for as long as a minute and a half (the original training was 10 seconds buzzer and one minute separation of buzzer and shock) the goat still continues to maintain flexion of its right foreleg as long as the buzzer is sounding. It will not, or cannot, take a chance. When the buzzer sounds for a minute or more the goat shows evidence of pronounced fatigue. Tremor of the flexed leg and gradual sinking of the forefoot towards the floor will be corrected by a sudden forceful flexion just before the foot touches the floor, as if from a hot griddle. Moreover, all sudden stimuli such as turning on lights, starting a movie camera, tapping the goat's side lightly with a wooden rod, instantly evoke a brisk and maintained flexion.

Sometimes the animal suddenly and spontaneously assumes the flexed position of the foreleg in the absence of any observable change in its laboratory environment. All alarms are now channelled to the right foreleg.

What keeps this simple and stereotyped avoidance response going when there is no longer anything in the animal's real situation to be avoided? It seems to us that this is fundamentally the same question which the psychiatrist faces in combating his neurotic patient's phobias. There seems to be no reason why the operation of 'traumatic memories' may not be inferred in both cases.

Several years ago we published definite evidences that the neurotic sheep takes its worries home to the barn at night. When a sheep is subjected to the monotony of a rigid time schedule of ten-second conditioned signals spaced six minutes apart, and this schedule is followed day after day, a severe emotional upset is precipitated and becomes chronic. The animal exhibits diffuse agitation in the laboratory, with frequent and vivid startle reactions, laboured breathing, and rapid irregular pulse. Even weeks or months after the tests have been discontinued the animal exhibits its perturbation in the barn at night. With the aid of a long-distance stethoscope the observer, in a shed outside the barn, can listen to the heart sounds of both normal and neurotic sheep. When the flock is resting quietly in the barn, the normal sheep's heart beats slowly and regularly; by contrast, the neurotic sheep's heart may be beating twice as fast with wide fluctuations of rate and with frequent premature beats. When placed on a long recording platform within sight of other sheep, the neurotic sheep, unlike the others, continues its nervous pacing back and forth on the platform during the dark hours, suggesting that its traumatic memories or worries have led to insomnia.

The examples just cited from our own research could be multiplied many times. I have reviewed them here because it is the purpose of this meeting to strive for the convergence and even coalescence of our diverse viewpoints.

Those of us who investigate animal behaviour with medical intent have been frustrated for many years by habitual attitudes of 'inter-doctrinal unacceptance'. This is a very happy phrase coined by Dr. Harry Kruse of the New York Academy of Medicine. For example, the statement is repeatedly made that the so-called experimental neurosis in animals can have little or no bearing on human psychoneurosis. This, it is said, is because the animal's emotional disorder is situational. It originates in the laboratory and appears only when the animal is returned to the laboratory situation, actual published evidence, such as reviewed above, notwithstanding. Thus,

'inter-doctrinal unacceptance' irradiates to embrace 'factual unacceptance', and that, we would agree, is going too far.

LORENZ:

That is what we are meeting for. You always find people who say 'I simply do not believe that'.

LIDDELL:

You publish it, give him the reference, and he still will not believe it.

FREMONT-SMITH:

Instead of saying 'I do not quite understand you'. I would like to suggest the term 'contra-doctrinal repudiation'.

LIDDELL:

The two traditions of research originating in the classical studies of CANNON (1936) and FREUD (1935) have, from their beginnings, converged upon the same fundamental problem, namely, the functional significance of emotion. This concerns us in facing the problem of the operational meaning of psychic trauma. This physiological tradition and this psychodynamic tradition will eventually coalesce in spite of the well-known inertia and conservatism of thinking peculiar to the life sciences. The merger will be hastened, I believe, when it is recognized that between the organic and the psychodynamic positions there is another distinct viewpoint--the behavioural or psychobiological position, which, I take it, this Group represents. Those of us who adhere to this position and who have persevered in the study of the animal's chronic emotional disorders brought on by the stresses of conditioning can contribute to an understanding of the functional significance of emotion. We can certainly do so when it is clearly recognized that a division of labour is based upon the well-established zoological principle of homology. This is the great contribution of ethology, that both Lorenz and Tinbergen have employed this principle of homology, and applied it to behaviour as well as to anatomical structure.

The sheep's foreleg and the human hand and arm are homologous structures serving diverse purposes. The sheep's foreleg is one member of a locomotor quartet and that is all. The human upper extremity is capable of incredible feats of manipulative skill. But the sheep's 'locomotor' brain and man's 'manipulative' brain are driven

to action by the same kind of primitive, neurohumoral 'emotional' machine.

If symbolizing derives from manipulating we cannot expect the sheep's simple but chronic neurotic disorder to reveal evidences of distorted symbolism or of the operation of a 'primary process' as in human psychoneurosis. But evidences of emotional disability obtained from sheep undergoing the long continued stress of conditioning and from soldiers subjected to the long continued hazards of the combat situation, as revealed in posture, movement, and organ dysfunction show them to be the same in sheep and man.

FREMONT-SMITH:

May I suggest that along with the concept of homology in behaviour, which seems to be absolutely vital to our needs, although we may not be ready to accept anthropomorphism, we should certainly be ready to accept biomorphism.

GREY WALTER:

Would you be willing to accept mechanomorphism rather than homology?

LIDDELL.

I was going to ask what term you would use. I thought when I saw your model I could put wool on it and have it in the pasture!

GREY WALTER:

Indeed you could. In the establishment of a conditioned defensive reflex it is very easy to show that theoretically and in the model reinforcement is not necessary. The defensive reflex particularly is liable to be self-reinforcing, and once established you need only press the neutral stimulus button, because of the feedback loop which I showed you.

LIDDELL:

In other words, your machine could actually, with proper circuits, do what the goat did?

GREY WALTER:

It does do what the goat does. The reinforcement of the conditioned defensive reflex is automatic in this very model, without reapplication of the specific stimulus. Before I brought the model

here, I wiped off all the labels I had put on it; I thought they were too provocative. The one which provides for the defensive loop was called 'obsession', because the thing was an obsession; there was no reward, it was a movement like a tic in human beings. This seems to be in essence the caricature of a defensive reflex. One of the questions I should like to ask later on is to what extent in animals and in men is there evidence of the unreinforced conditioned reflex based on repetition.

LORENZ:

That is what I was going to ask: whether you get this kind of independence of reinforcement in anything other than avoidance reactions. It looks very like a neurosis, and looks very disorderly, as if the animal becomes obsessed about some stimulus. Now from the naturalist's point of view, which I am always trying to keep parallel with the causal investigator's point of view, it is of enormous survival value that the animal *does* become obsessed. It would be very inadaptive from the point of view of survival value, if a goose, which has once escaped some predator, did not become obsessed about this method of escape. If after a time he became curious or tried to find out whether that predator really was dangerous this would be inadaptive.

So I think there is some difficulty, as there always is, in drawing the borderline between the still adaptive and the pathological. I wondered whether your goat, which retracted its leg, and reacted with this escape reaction to every stimulus, was not far nearer to the adaptive than the sheep who runs about at night?

LIDDELL:

It is my particular point of view, which I think we can now justify, that all Pavlovian conditioning is stressful conditioning, unlike the Skinner box. I was led to this view by my unfortunate beginning in behaviour research when I attempted to employ the methods of the animal psychologist in attacking the thyroid problem of Dr. Simpson. As I mentioned a few days ago, he wished me to match the common signs of hypothyroidism, slow pulse, retarded growth, muscular slackness, and so on, with a mental dullness which occurs even in the human adult suffering from severe hypothyroidism. Being at a loss how to attack this problem, I found in an old *Journal of Psychobiology* (when the *Journal of Animal Behaviour* was discontinued with the outbreak of World War I, it never resumed publication; for a brief period, under the influence of Professor Knight Dunlap, a journal appeared called the *Journal of*

Psychobiology) an article by K. S. Lashley and Shepherd Ivory Franz which was the original article of a series in which they sought to show the effect of cortical extirpation upon the learning of the rat. They employed a very simple T-maze with the arms bent down, and I modified this as an outdoor maze to test normal and thyroidectomized sheep and goats. Here I discovered (to my consternation), that a repetitive behaviour pattern went on of its own momentum without reinforcement.

Briefly the circumstances were these. We built a board fence about an enclosure with three parallel alleys, the middle alley bounded by wire screening. There was a little starting compartment with a door which would fall, allowing the animal to emerge. Then the animal had to find its way down one of either of the outer alleys in order to reach the final compartment, at the end of which there was an iron grill door, leading to the barn, where the flock of sheep were. There also was a small box filled with oats. Either alley could be made 'blind' by the experimenter shutting a door. Now the animal, as I naively supposed, under the motivation of its gregariousness, wanting to join the other sheep, and secondly motivated by its desire to have a bite of oats, must learn to avoid the gate of the alley which was closed (the cul de sac). It would come down this alley, find it blocked, retreat and go round to the open alley leading to flock and food. The animal would learn to do this. Then we could reverse the position of the cul de sac or blind alley, and it would learn to go the other way. Now, to make a long story short, I never demonstrated any definite difference between normal and thyroidectomized animals; even though the thyroidectomized animals were sluggish and only one-third the body weight of their twins, I failed to detect an intellectual blunting—if you want to call it so. But in the course of these events I came upon what seemed to me most disconcerting facts about repetitive behaviour. I finally found a problem which no sheep or goat ever solved, namely a temporal alternation in which the animal, on its first trial of the day, must turn right to get into the final compartment and in the very next trial (it returns at once into the starting compartment) it must turn to its left, then to its right then to its left; four times in succession. The animals never learned to do this. All normal sheep and goats could do the first two trials without error, but the third time they would hesitate and sometimes make an error, and more often still make an error on the final trial.

I continued with about eight sheep over a period of three years with three runs a week. They were very willing indeed to do this, but none of them ever mastered it. But the results were dreadful from my pedantic point of view. Neither gregariousness, nor the desire to eat, had any influence on this performance when it was well

established. This is what would happen: the sheep, as soon as the trap door fell, would deliberately go down the central alley, dash down the open alley to the final compartment, make a wheeling motion, ritualistically rub its snout through the oats, not taking a single bite, and go and wait to be let into the starting box for the next trial. Then it would turn in the opposite direction, and in spite of making mistakes it was so anxious to get back and try once more and get the matter done that it would not eat. It would go through the motion of eating, but would not eat actually. Then, to check further, we put a wooden storm door between it and the flock to eliminate the operation of gregariousness, but the sheep was by no means concerned at not finding the flock, it wanted only to get back and repeat its maze performance, in spite of its errors.

Still worse was to follow. One of these ewes had a lamb, and out of deference to her condition we fenced her in a little screened enclosure in the barn. I should have told you that not only did the sheep ignore the flock and not bother with oats except for the ritualistic rubbing of its snout along the surface of them, but when I came in at the usual time of day the animals would be clustered around the maze door waiting to be let in. They were anxious to be run. Then when this ewe had her lamb, we put her in the enclosure to suckle the lamb with the flock in the barn, and one day (permit me to anthropomorphize) as I came by she bleated at me with intent, or so I interpreted it. I let her out of her screened enclosure, her lamb trotting along beside, and led her into the maze with the iron grill door separating her from the lamb. She promptly ignored the lamb, which bleated at the top of its lungs and threw itself against the gate. It made a great uproar, but mama ignored it completely and ran her usual four trials through the maze.

GREY WALTER:

I would say this is love not appetite.

FREMONT-SMITH:

Love of you, seeking approval of the experimenter.

LIDDELL:

I do not think so really. I was up in the loft of the barn making observations. It might be so, but I had no evidence that the sheep had so strong an affectional bond.

LORENZ:

Had they run the course very often?

LIDDELL:

Three times a week for a period of about three years.

LORENZ:

There is one thing that bothers me very much. It is one of the things that ought not to be from our point of view, but is. Learned behaviour develops something like an autonomous appetite for it! If this learned behaviour is 'ground in' very deeply, the subject will develop a very strong appetitive behaviour towards it. I can give you quite a number of instances when the same thing occurred in other animals. For instance, sledge-dog mothers often completely ignore and leave their litter in order to join the team, trying to get into their accustomed place and wanting to go on pulling the sledge.

LIDDELL:

The reason I wanted to bring this up was to hear of your own experiments on work drive.

LORENZ:

The most anti-militaristic man, who has hated being trained, will try and see if he can make militaristic movements too if you give him a gun.

GREY WALTER:

Might this not be an example of the natural confusion which exists, particularly in higher animals, between the various forms of adaptation, that some sort of practised learning may here be superimposed upon associative learning, which you were intending to study? By this very prolonged repetition of an act, which you say was never quite completed, you have simply a ball running in a groove not going anywhere at all except following the gradient.

LIDDELL:

Except for the zest with which they lined up and wanted to be let in.

GREY WALTER:

They actually performed some practised act, however, which may have been associative learned behaviour.

LIDDELL:

I think in favour of what you said is the fact that the dwarf, sluggish, thyroidectomized animals ran with the same persistence as

the normal animals, but did not line up at the door; they were passive and inert, and did not have the zestful approach.

FREMONT-SMITH:

Perhaps there is an element of fulfilment of anticipation in this situation. Here you have anticipation of going through the maze, and dissatisfaction until it could be fulfilled.

LIDDELL:

One additional factor I would like to put in the record, showing that you cannot come to any facile chain-reflex or chain-response conception of this repetitive act. I could take the sheep out of the maze and into the barn and allow him ten minutes to cool off anywhere in this series of four trials, and he would take up the next trial in series almost always without error. If he had done his first turn, and turned to the right, then I would haul him out and leave him in the barn for ten minutes, I would return him after this and the very first turn would be to the left, which was correct. So it was nothing so simple as a reverberating time-circuit or kinaesthetic feedback. It was much more complicated.

WHITING:

I am not quite sure of the design of the experiment. When the animal went to a blind alley, exactly what happened? Did you open the gate to put him in?

LIDDELL:

When he found his way into the final compartment, he would automatically wheel and wait at the gate to be let back in the starting compartment.

WHITING:

How was he let in?

LIDDELL:

By me.

WHITING:

Did you touch him?

LIDDELL:

No, I walked around him and opened the gate.

WHITING:

You were there?

LIDDELL:

I was there.

GREY WALTER:

Then you were the reward.

LORENZ:

I want to emphasize that Liddell was not the reward, the running of the maze was the reward. You can see these learned movements develop a strong appetite of their own in circus dogs escaping and running through their loops. When I was a prisoner of war we taught wild mice to run in wheels. We kept these mice in an area between the double windows. They gnawed through the wall and escaped. There was no longer any food in that area, but those mice went back every night to run in that wheel. We could hear the wheel squeaking, and that went on for months and months. They were not fed, there was no recompense at all, but they had learned to run in a wheel and they liked it. I always disliked doing anatomical dissections, but when I see a man dissecting something I say, 'Let me do that for a moment'. That is a phenomenon which is very interesting, because it puts up an analogy between learned behaviour and instinctive behaviour, which is very disturbing to me. It is quite opposite to what we expected. And because it is very disturbing it should not be forgotten for a moment!

WHITING:

I think it is crucial to exclude all the possibilities of the result being accounted for by some reinforcement principle before we say that it is due to some principle of interaction. Would the sheep eat oats after the fourth trial?

LIDDELL:

I cannot remember. I know for the other trials that in his impetuous zeal to get back into the maze for the next trial he would go

through the motion of rubbing his snout across the oats. I imagine in most cases he would eat after the last trial.

WHITING:

A possible explanation is that the four trials *altogether* was something that had to go on, after several months, in order to get the final combination of running four times, of eating the oats, of getting back into the flock and getting the evening meal.

MEAD:

Would you still have in your record what that sheep did at the end of the fourth trial, whether it did or did not eat?

LIDDELL:

I could not be sure. I am certain that the sheep always waited for me at the fourth trial.

FREMONT-SMITH:

But did it wait any longer at the fourth trial than at the second or third?

LIDDELL:

He did not go back to the starting place, he would be waiting to get into the barn. I do not remember if he ate or not; but there was a terminative pattern of behaviour—‘Done at last!’

GREY WALTER:

It is very hard to design experiments where there is not some reward.

BINDRA:

It seems to me that the mechanism underlying this positive sort of compulsion in the sheep is probably quite different from the mechanism of compulsion in the escape or avoidance situation. Even in the rat you can get compulsive avoidance established very quickly: if the rat is able to avoid a shock by pressing a lever, he will keep on pressing the lever and keep on avoiding the shock almost indefinitely. But another thing that happens is that the rats periodically ‘test’ the situation. Once in a while, every thousand trials, or

maybe every two thousand trials, they do not press the lever and actually get a shock; it seems they are testing to see if the shock is still there.

LORENZ:

The rat, dog or chimpanzee would, after a time, try to investigate whether the danger was removed, and so would the child or any of what I call 'curiosity creatures'. But the wonder is that sheep will not; they are more conservative.

GREY WALTER:

At a certain stage in evolution you get the question asked, 'Is your obsession really necessary?'

BOWLBY:

At what age were these sheep started on their training?

LIDDELL:

They began their training at about three weeks, not in this complicated problem but in the simple maze with the cul de sac in just one position.

BOWLBY:

The question in my mind is whether this tendency to develop an autonomous drive in learned behaviour develops more frequently if the creature is taught the procedure when it is young, rather than when it is old or middle aged.

LIDDELL:

What, in corresponding terms to the human life span, would be the age of the sheep at three months?

LORENZ:

In that respect they become more complicated than the dog. The sheep is born at what would correspond, in humans, to the age of four years.

LIDDELL:

We get perfectly precise conditional reflexion at seven days.

LORENZ:

The lamb is able to walk and travel as fast as his mother at about five days old. He can follow her easily, which a child would not be able to do before five or six years.

BUCKLE:

It seems to me this kind of behaviour which developed in the sheep is what we would call in humans an example of sublimation. That is, of getting an autonomous drive to do something which is not reinforced by the original instinct pattern. We think it is not reinforcement which produces sublimation but the frustration of some instinct or action. We do see that the sheep has been frustrated at the beginning of this experiment because he has come up to the flock and the gate between him and it has been shut. Could one perhaps investigate variations in regard to the facility with which this autonomous kind of maze-running drive develops?

LIDDELL:

I do not remember the sheep trying to force its way into the barn to join the flock. There was no frustration in that sense.

GREY WALTER:

There is one point about this with regard to humans. Electro-physiology is still very undeveloped, but in the human being at about an equivalent age of the sheep, around four years old, as I think I mentioned at our last conference (Vol. I) the termination of pleasure or an agreeable stimulus (as opposed to the application of pain) has a specific action on the brain. In children aged about three or four years it seems that frustration from the withdrawal of a pleasant stimulus has a more profound and long-lasting action than application of an unpleasant stimulus.

BUCKLE:

I gather this is just one of the points at which human psychologists and animal psychologists do not get together very much. We deal largely with children and deprivation effects, and most animal psychologists think all the time in terms of reinforcement theories. 'Where is the reward?' is usually the question they ask.

MEAD:

We have a concept developed by Professor Boas in the understanding of primitive art which he called 'the play of the virtuoso

with his technique'. We find a great many instances where there are repetitive, highly-skilled and perfected pieces of behaviour which are not displayed. For instance, in the fringes of coats made by natives on the Amur River there is a beautiful rhythmic colour arrangement which is put in the fringe at a point where no one can see it. You have to take the coat apart to find it. This is fairly common. Boas' explanation was that there was a self-rewarding pleasure in doing something well, which accompanied doing it habitually. I would think in setting up the experiment we ought to consider that there is no deprivation element in this except the deprivation of the pleasure of doing it. You will find with children who are spinning a top or bouncing a ball that stopping the action is the deprivation, not taking the ball away.

LORENZ:

I think that the task must be very difficult to the animal in question in order to offer this particular kind of satisfaction: a rat would never think it 'good sport' to run a maze, because it would be too easy for him. To the sheep it is very difficult, and that is why it becomes a pleasure.

LIDDELL:

Sometimes I thought of children lining up to go down the slippery slide over and over!

Going back to earlier experiments I did, inadvertently, stumble upon a procedure for establishing real frustration, and it suggested, perhaps, the origins of what in ourselves might be psychological traumata. To save time I shortened the maze so that the animal could perform his repetitive task by just turning round the corner either to left or right. Then, as always happens in animal experiments, the experimenter made more mistakes than the animal. On the third trial of a four-trial sequence I inadvertently left both gates closed. The normal sheep turned correctly but was startled to find a closed gate, so it stopped, turned but came back and looked again and then was very confused and began bleating which it ordinarily never did in the maze. I went down and corrected the error in position of the gates, but the next day, twenty-four hours later, the sheep went through its usual routine until it came to the third trial, where it stopped in its tracks *just before* the place where the gate had been closed by accident the previous day. That suggested a further experiment. Later I fired a revolver at this point on the third trial of one day and the next day the sheep suddenly stopped and then dashed about as if confused and alarmed. This startled behaviour suggested

the influence of vivid traces of the formerly blocked sequence of actions.

LORENZ:

May I come back pedantically to a question originally put by Grey Walter? People who believe in the self-rewarding quality of well-known things agree on one point, and that is that the whole mechanism of this obsession is different from the obsession to negative stimuli. Consider what would be the equivalent to an obsession with a negative stimulus if it ever happened with a positive stimulus: supposing an animal has once found in a definite place in a definite situation something particularly good to eat. It would then, never, never be able to learn not look in this place for something more. This would be very unadaptive; we come back again to survival value. Of course, the causal explanation is not solved by that, but we find some reason why a special construction within the central nervous system might have evolved which makes it easy to get obsession to negative stimuli but prevents an obsession to positive stimuli.

But let us examine this. I had a muscovy drake, who had never performed the consummatory act of copulating with a goose. The apparent object-fixations of sexual impulses are really most exactly conforming to the definition of an 'obsession' because they seem to be really irreversible. This is very much like a real object-fixation in humans and, with all caution, I think they possibly might be simply the same thing, though it is for the psychoanalyst to decide that. In this case you had got that muscovy drake exposed to a certain stimulus without even a consummatory act, but nevertheless he kept on to 'look in that place'. Yet I think the mechanism is different from that of the 'obsessions' found by Dr. Liddell.

WHITING:

I think there is something we should take note of in these differences between negative and positive situations. One is that in the negative situation there is a rapid onset of the pain, and equally possibly a rapid cessation. This is contingent on external events. Your positive stimuli depend on slow metabolic processes and a slow onset, and generally have a slow decrease.

LIDDELL:

You would also say, would you not, that the goat touching the fence with its muzzle, stopping again to flex the foreleg and then

running is not at all the same as the sheep suddenly stopping in the maze? Here we have detachment from the original situation.

LORENZ:

Definitely.

LIDDELL:

When we first began to study conditioned responses in animals, we attempted faithfully to duplicate PAVLOV's (1927) procedure. Pavlov's dogs had been caused to stand upon a laboratory table for a very practical reason, namely that he was interested in collecting gastric juice from a gastric fistula, and this table and restraining harness were for the experimenter's convenience in putting a flask under the belly to collect gastric juice and take it away. So, through laboratory tradition and habit we persevered in the unhandy procedure of making the sheep climb a ramp and stand on a table in the conventional Pavlov frame.

We started on the conditioned reflex work with the sheep and goat, because we wanted to cross species lines. We also worked with the 'salivary' dog.

Finally, we drastically simplified the whole conditioning procedure. At present all that is done is to lead the animal into a place against the wall where there is a metal cleat, and put the web strap around its chest and through the cleat. Of course, the animal will struggle at first. Finally, he will decide to stand quietly waiting for the signal and for the brief mild shock upon the foreleg which follows.

Now I want to discuss how our present attitude towards Pavlovian conditioning originated. I first visited Pavlov's laboratory in the summer of 1926 where I met Horsley Gantt, who had gone over with the famine relief after World War I and had stayed there six years working in Pavlov's laboratory. I arrived in Leningrad and was put in touch with Dr. Gantt, and to him I am eternally grateful for rapidly indoctrinating me in Pavlov's laboratory tradition and enabling me to meet the most important people, one of whom was Dr. Grey Walter's friend, Rosenthal.

In the summer of 1929 Dr. Kupalov was sent to A. V. Hill's laboratory as a Rockefeller Fellow, and I was able to borrow him from the Rockefeller Foundation. We spent the summer of 1929 in my laboratory and then went together to the International Physiological Congress in Boston late that August where I met Pavlov for the first time.

Parenthetically, Dr. Grey Walter, I must say that Dr. Kupalov told me why Rosenthal wanted very much to come to Cambridge

and why Pavlov wanted him to come was that they wished to establish conditioned reflexes in the British bulldog because of his steadfast and indomitable character, which reminded them of the British! Did he ever work on the British bulldog?

GREY WALTER:

None of our dogs were bulldogs. In the end he had to bring dogs from Leningrad!

LIDDELL:

It is due to Kupalov more than to any one of Pavlov's whole group that I am indebted for getting a new view of Pavlovian conditioning. First of all, Kupalov had convinced himself, and he easily convinced me, that Pavlov's conditioned reflex procedure did not effect what Pavlov purported, namely that the dog lost his independence of action and, as soon as you reduced the environment to a sum of controllable stimuli, all spontaneity of action would disappear and you would have perfectly predictable suites of reflex actions, conditioned and unconditioned. Kupalov at that time told me of an experiment which Pavlov hesitated to publish and, indeed, never did publish (and this has led to Kupalov's undoing at the present time because he has been, as we say, in the dog-house since faithfully reporting this experiment). He told me that the following amazing circumstance occurred in Pavlov's laboratory which disturbed the old gentleman very much indeed and delighted Kupalov. It was as follows: The dog was standing quietly in the harness on the table. The observer was looking at his manometer and suddenly he saw the fluid in the manometer begin to move. This meant that the dog had begun to secrete, *and that was against the rules*—he had no business to be secreting because the experimenter had given no signal. This dog had been trained to expect food when an electric light bulb in front of him was turned on, secondly when a metronome on the beam over his head began to click, and thirdly, when an electric tapper under the edge of the table began to work. The experimenter looked through the periscope when this secretion began and he saw the dog peering over the edge of the table wagging his tail and salivating. Signal number two was given with its usual stereotyped conditioned response. Later in this period the manometer fluid began to move again—secretion! The experimenter looked through the periscope to see the dog leaning towards the electric light bulb wagging his tail and salivating. The third time the dog looked up behind him to where the metronome might have been clicking. Kupalov, as I say, was undone by referring to this as a

conditioned reflex *without initiation*. Here then is a conditioned reflex with no stimulus.

It seemed that this conditioned reflex behaviour, which we had all read about in the literature, was dramatized dog behaviour: the stage setting was such that it emphasized stimulus-bound behaviour. But here the dog in the stimulus-bound situation was exhibiting *spontaneous behaviour*. How could this be, if the dog was a passive agent in the clutch of environmental stimuli? Then Kupalov made a second suggestion which has coloured all our future work, and it stemmed from the following experiment in Leningrad.

One tactile stimulator was placed upon the dog's thigh, and another on his chest. When the tactile stimulator pressed lightly upon the shaved spot on the chest once every two seconds for thirty seconds, the dog was fed. Exactly six minutes later the spot on the thigh was stimulated for thirty seconds, no food following. Six minutes later, the positive spot—food. Six minutes later, the negative spot—no food. So there was an oscillatory plus-minus pattern of constant intervals, with respect to time. Kupalov then decided to see if there remained in the nervous system any residuals of this rhythmical temporal pattern by changing to a luminous circle as conditioned stimulus and allowing this to shine for thirty seconds, feeding the dog, and then six minutes later presenting the circle again followed by food. He kept the same time-pattern as before; but now, he found that the dog very soon reacted to the circle with large secretion on the first trial, a small secretion on the second, a large one on the third, a small one on the fourth, and this gradually damped out until the salivary responses became equal for successive presentations.

It occurred to him, therefore, that since the dog's central nervous system could be thrown into a state of readiness in step with repetitive stimuli, this might be a convenient means for bringing about an emotional crisis (experimental neurosis) in the dog. This proved to be so. We adopted Kupalov's procedure and found that in both sheep and goat this was the most convenient means for bringing about chronic emotional disorder. We gave our sheep or goat a ten-second signal and then waited two minutes before the next signal. Thus: buzzer—shock, two minutes; bell—no shock, two minutes; buzzer—shock, two minutes; bell—no shock. We found that this was a very stressful procedure for the sheep and goat.

We then went a step further and found that it was unnecessary to use any negative signal; all that was necessary was to give a ten-second signal followed by shock, wait a constant interval, another ten-second signal, another shock, and so on—positive stimuli separated by equal intervals of time.

This led us into a problem which, I think, will be of great interest to Dr. Grey Walter in the next model which he constructs; this is the question of time estimation. We have found the sheep and goats are adept at estimating time intervals up to seven minutes. We do not know what the outside limit in the sheep and goat may be, but we found in Pavlov's book (1927), that if the dog were fed every half hour, and you missed a half-hour feeding, he would start secreting, with an error of two to three minutes either side. But if you focussed his time perception by giving him a readiness signal, that is if you sounded the metronome thirty seconds before the half hour and gave the food, the dog had developed a much sharper time sense and now he would not secrete a drop if the metronome were thirty seconds too soon.

We found the same phenomenon in sheep and goats. If we stood the sheep in the Pavlov frame and gave him a shock every six minutes, he would get restive maybe two or three minutes before the six minutes; and if the shock failed he might maintain his restiveness some seconds afterwards. However, if we preceded the six-minute signal by a ten-second signal, just as in the case of Pavlov's dog, the time-perception became very much focussed and sharpened. This added signal is the straw that breaks the camel's back. You do not get experimental neurosis by marking off the equal time-periods by just a forced reaction to shock. You must give him, one might say, an anxiety signal, a readiness signal, and it is this readiness signal that does the harm.

Without fail, therefore, in any sheep and goat we bring about emotional catastrophe by forcing their reactions into a rigid, Procrustean bed of constant timing, providing the readiness signal precedes the shock.

We can do more: we can still further increase the stress situation by allowing the animal to avoid the shock by flexing his leg at the appropriate time, but then giving or withholding the shock in a random manner.

MEAD:

The signal was monotonous and the shock was jumbled?

LIDDELL:

Yes, we know that monotony is neurosis-producing; the jumbling effect is adding fuel to the flames; you get more rapid neurosis in that way.

HARGREAVES:

Is this really monotony? Is it not repeated apprehension?

BINDRA:

I do not think I have understood this quite rightly. Did you not say that monotony by itself might produce a similar set of reactions?

LIDDELL:

No, the correction is important; it must be monotonous apprehension.

GREY WALTER:

Might it be the incongruity between the regularity of the stimulus and the randomness of the punishment?

LIDDELL:

With inevitable shock the monotonous apprehension *per se* brings on neurosis after some weeks or months. But you hasten it by randomizing the shocks. This is in sheep and goats.

GREY WALTER:

It is a very special case in the sheep and the goats. It certainly is not true in the dog; our dogs gave no sign of minding monotony *per se*; I think they rather enjoyed the drill.

TANNER:

Does the sheep after this procedure get the aberrant patterns of heart action during the night that you told us about, and similar symptoms?

LIDDELL:

We have yet to determine that. I must frankly state that we took too histrionic a view of experimental neurosis. We thought it must be dramatic behaviour, which anyone can see. We now believe that many instances of neurotic, emotionally disordered behaviour, may be as subtle and as easily escape detection as human psychoneurosis.

GREY WALTER:

But the neurotic animal was able to feed and mate in a normal way?

LIDDELL:

That is subject to inquiry. We have reason to believe that one of our so-called normal rams was psychologically impotent. Ordinarily, we have one billy goat brought in for the mating season and one

ram. On this occasion we did not bring in another ram but our own ram did not impregnate any of the ewes at breeding season.

BINDRA:

In normal feeding conditioned responses you get general inhibition during the delay period, at least so Pavlov (1927) reported. Do you get any similar thing in avoidance responses?

GREY WALTER:

Yes, you do in dogs.

BINDRA:

The animals will go to sleep?

LIDDELL:

Did you observe sleeping in your experiments?

GREY WALTER:

Yes, in a certain type. This is where I have to make a reservation. There is a typological difference here in dogs. These particular factors—the development of a disturbed behaviour in relation to randomization of stimuli and the extent to which you get sleep or excitement during the delay period of a delayed reflex—are factors which are enormously affected by the individual type of dog. Before typology was thrown overboard that was the main subject of interest in the Pavlovian Institute. These typological differences could be distinguished genetically. The last that was heard, before the work was jettisoned, was that they did breed true. Some animals do go to sleep in a delayed reflex situation, when a quarter of an hour later a violent shock is to be given; but other animals will become agitated and neurotic.

LIDDELL:

This is interesting, because when Kupalov joined us he faithfully observed the sheep and goats day after day. We also had a dog laboratory for salivary-dog conditioning, and we did get the hypnotoid type of behaviour in one dog we were working with. Kupalov diligently observed the sheep and goats for signs of somnolence, and it did not occur. He was quite surprised at this species difference.

Before proceeding further, I must say that we spent a great number of years faithfully reproducing all the principal phenomena of conditioned reflex action which Pavlov had described in the dog,

as they appear in the pig, in the sheep and in the goat. Pavlov was an honest and gifted observer; all of these phenomena can be reproduced, even irradiation, and concentration of tactile positive and negative conditioned reflexes on the sheep's thigh. Most of Pavlov's phenomena *are* transferable over a species range.

First of all, we believed that the Pavlovian conditioning situation was essentially a stress situation, and we tested this notion in two ways. In the first place, we conditioned a sheep until we had a well-established conditioned reflex to buzzers based upon shock. We carried on training to between one and two hundred buzzer signals. The animal had quietened down and stood alertly between signals. The signals were randomized with respect to time. In this experiment we were recording respiration, head movement, leg movement, and heart rate. We then carried out the following test. The animal was brought to the laboratory at the usual time every day and placed in the Pavlov frame; the usual apparatus was attached and a graphic record taken of the sheep's standing behaviour. In two months this animal might almost have been used for a basal metabolism test as it stood in its frame. The whole set of physiological functions gradually diminished in intensity until the animal was quite at ease, quite relaxed. Then, after about three months, a single buzzer signal threw him into a most violent and uncoordinated struggle to escape —almost what you observe when the animal has reached the neurotic state. So by habituating the animal to standing without stimulation in this restraining harness the tension-arousing nature of the restraint gradually diminishes and the animal is at ease.

The second experiment was this. We brought the animal—in this case a goat—into the demonstration box (as in Fig. 15, facing p. 65) for his daily demonstration, attached the electrodes and then sat and watched. The animal, with no signals and no shocks, standing at a familiar time of the day, became more and more excited as the hour progressed. The excitement took the form of increased respiration, beginning at about 45 per minute and ending at 135 respirations per minute. The breathing became more and more disturbed; there was teeth grinding and yawning, and fidgeting movements with the leg. If such an animal is let out to pasture for an hour, and two hours later brought back, you find that two hours at pasture by no means alleviates this tension. He now starts the afternoon session almost as disturbed as he was at the end of the morning session where we left off.

BOWLBY:

By using the word 'shock' we are in danger of thinking it is a very disagreeable stimulus. Frustration of the motor response would appear to be what is unpleasant.

LIDDELL:

We like to speak of it as an electrical stimulus, and that is all it is. As Dr. Grey Walter mentioned earlier, individuals vary enormously in their experience of the disagreeableness of a minor electric shock. Sheep and goats have almost an innate releasing mechanism to escape from a shock which none of us would feel, or at most feel as a slight tingling.

MEAD:

The animal gives a startle response rather than a persistent, long attempt to avoid, as it would with pain. Can you use this stimulus with this degree of strength and train the animal to avoid it and to avoid situations where it occurs?

LIDDELL:

Yes, this happens in two stages. At first every normal sheep and goat, from a week of age up to maturity, when placed in the restraining harness and given this mild shock, makes violent struggling and plunging movements, and if free in the room will dart away. Then gradually, by habituation, the startle reaction becomes a very perfunctory reflex of the stimulated limb.

BOWLBY:

If the same stimulus is applied in other parts of the body, what is the reaction?

LIDDELL:

Escape. But if you apply it within the apical part of one foreleg it will become a perfunctory reaction and give you the illusion that you have a definite, precise defensive reflex.

GREY WALTER:

In the dog you get snapping. A small stimulus, not a painful one, to the leg or arm of a dog will not produce an escape reaction; most dogs will just snap.

LIDDELL:

It occurred to us to see if we could not study motor-conditioning in a free situation where the animal, if it wished, could have freedom of locomotion. In this case we chose twins, three weeks of age, one with the mother and one without. The twin was connected by a

flexible cable from the centre of the ceiling, and this cable brings down the electrodes to the legs so that the shock may be delivered. In all the experiments which I shall be describing from now on the signal was the turning off of the lights in the room; the conditioned signal was darkness. In this way, one experimental unit would not interfere with the work of another; the signal would not leak from one room to another. The lights go out for ten seconds and then the little animal gets a shock on the foreleg. This is repeated every two minutes for twenty darkness signals and then the animal and its mother are led from the room. Meanwhile, the twin in the adjoining room without the mother is similarly connected with a recording system. In a very short time, part way through the first period, all communication by bleating from room to room is given up. The mother gives up first; the lamb bleats persistently but the mother does not reply, and soon the solitary animal has given up vocalizing.

The recording system is simple. The flexible cable from the animal leads to two selsyn motors on the ceiling, and the sympathetic selsyn motors in the corridor record the progress of the little animal about the room by moving a pencil over the page of a notebook. One gets a locomotion chart for the whole period of twenty signals.

The little animal's reaction to the very mild electrical startle stimulus is to rear and dash away, then it comes back to its mother. But after ten or fifteen darkness signals each followed by shock, the animal gives the precise conditioned reflex in the presence of the mother that we have seen in the animal confined by the strap to the cleat on the wall; that is, the simple foreleg movement. In the puppy you will not get this reaction until it is about twenty-eight days old, when a precise foreleg movement is first elicited.

LORENZ:

But the animal does not avoid the shock?

LIDDELL:

No. In the experiments I am speaking of all shocks are inevitable.

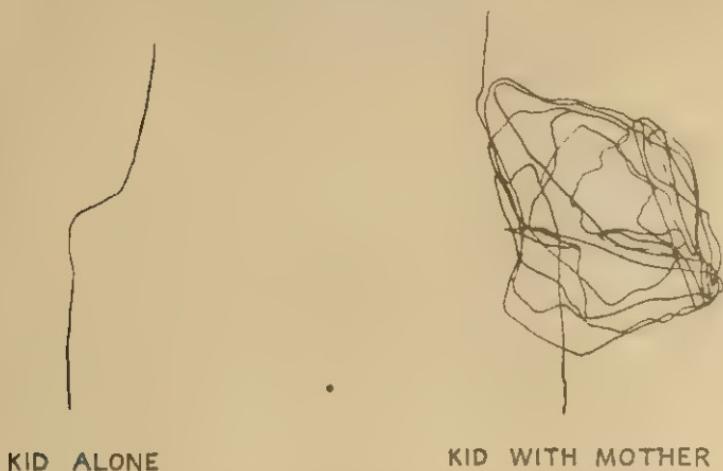
LORENZ:

So that leg-lifting develops even if it has no influence on shock avoidance?

LIDDELL:

Yes. We have done the corresponding experiments with the goat with practically identical results.

FIG. 16
RECORD OF ACTIVITY RECORDED BY SELSYN
MOTORS DURING 20 SIGNALS



KID ALONE

KID WITH MOTHER

Fig. 16 is the activity chart for the whole period of twenty signals with two-minute intervals for the kid alone and the kid with the mother. Now see what is beginning to occur. The solitary animal is in a temporal strait-jacket, with every two minutes the stimuli occurring. The animal with the mother wanders around the room in between the shocks. It will go and stand by her, typically, and flex the leg precisely at the signal. Then perhaps it will cuddle up next to her during the interval. The observer's chair is in one corner. The solo kid or lamb soon begins to orient towards the experimenter, and it creeps along the side of the room from trial to trial.

FREMONT-SMITH:

You are always in the room then?

LIDDELL:

In the case of this particular chart, yes.

MEAD:

Is the experimenter in the room with the mother and the lamb?

LIDDELL:

Yes, an experimenter also sits in the room with the mother.

MEAD:

In the same position?

LIDDELL:

Yes, but apparently the mother reassures the lamb. It may come up to the experimenter and trot away. The solitary lamb or kid comes hesitantly up to the experimenter's chair.

LORENZ:

The diagram on the right seems to show that the room is bigger on the left side.

LIDDELL:

That is the distortion of the recording system. It is not mechanically quite accurate. In our recent experiments we had a one-way screen for each room. The experimenter was not in the room then. In that case, the animal might take a position in any corner. But the same restriction of activity along the wall occurs.

LORENZ:

Along all the wall?

LIDDELL:

Yes. Is it not true in psychopathology that occasionally patients will creep round the walls and fear the centre of the room?

HARGREAVES:

In the film made by the French group who are doing separation studies, you see that very clearly. The children move round the walls with their faces to the wall and their backs to the room.

BOWLBY:

Supposing the same experiment was carried out without any 'shocks' or even without any stresses, would the solo lamb remain more stationary than the lamb which is with its mother?

LIDDELL:

It wanders around, once it is reassured about the experimenter.

LORENZ:

I would say it would wander about more than the lamb with its mother; but I wonder what the mother does all the time. Does she wander about much?

LIDDELL:

She wanders very little. She is bored by the whole proceeding. Typically, she comes in and lies in one place and grooms herself. Her baby comes to her and may jump on her at the shock and cuddle up beside her in the intervals, but she is relatively indifferent to the baby.

ZAZZO:

Have you in some cases made the experiment of separating from the mother the lamb that is normally with her? I should like to know whether in such a case one would observe the same phenomenon of keeping close to the walls as for the twin that is usually separated from the mother. Would the tangled pattern on the right come to resemble the pattern on the left if the twin that is usually with the mother were accidentally or occasionally separated from her?

LIDDELL:

We have not tried that.

BINDRA:

Did both the lambs have the same experience with the mother before the experiment?

LIDDELL:

Yes, they are with the mother the whole time except for this brief period every day.

TANNER:

Does the behaviour of the two young animals differ in relation to the mother in the pasture and in the barn when this has been going on for some time?

LIDDELL:

Yes; there is a difference. The little lamb will be loth to follow its mother and twin into the laboratory. Previously, it has trotted along

behind the mother; now it lags behind her. A separation is already occurring, a detachment from its mother, which is observed by visitors.

MEAD:

In the work that Erikson (1951) did in California, when he was studying projective block-play of pre-adolescent girls and boys, the wall was the symbol of the mother in the girls' constructions.

LORENZ:

That is what I also wanted to say.

LIDDELL:

You think that perhaps contact with the wall is like that with the mother?

LORENZ:

I think that the lamb which is without its mother and which is missing its mother has a stronger urge to cuddle, and, lacking something better, it cuddles the wall as a substitute.

LIDDELL:

I think from the behaviour that your supposition is correct. During the whole period the experimenter is a source of ambivalent reactions on the part of the young animal, and in the early stages when it is more mobile it will run towards him and try to wedge itself behind his chair. Then he makes it take its distance and it will approach him when the light goes out, wheel and run away and then come back. Once we wanted to be suprascientific and had wire recording to get the vocalizing of the animal, but we did not see the experiment. One of our lady assistants, a college girl, was sitting in the room as observer. When we ran the tape recording there was silence—then an irate girlish voice saying, 'Dammit, get off my lap!'; so that was the end of our wire recording.

Now I want to report a further experiment. We selected four pairs of twin goats. One of each pair of twins was taken from the mother at birth and raised on the bottle in an orphanage which was a fenced enclosure where they could see neither sheep nor goats. The other twin with the mother was normally raised in the barn and out at pasture. Each day for fifty days, starting at three weeks old, a dual experiment was performed, the one little goat coming with its

mother, the other little goat brought from the orphanage to the experimental room and never seeing its mother at all. All were tested by the schedule of twenty lights-out signals, of ten seconds duration each and separated by two minutes. The experimenter watched through a screen. At the end of fifty of these sessions, in which, therefore, each little animal had been subjected to one thousand darkness signals, separated by exactly two minutes at twenty signals per day, the procedure terminated. The four little goats in the orphanage carried on their orphanage life, and the other four goats were out with the flock as before.

GREY WALTER:

Were the four orphans together in their orphanage?

LIDDELL:

They were together. We wanted to re-test these four animals trained with mother and the four trained without mother at the end of two years; but at the end of the first year, we resolved to allow the four little goats from the orphanage to rejoin the main flock. We carefully arranged this. We brought the large flock of fifty to sixty goats into our rather large barnyard, and then we brought the first little goats from the orphanage and turned them into this paddock. They would have nothing to do with the rest of the goats, but followed their leader, a female, into a little alley. The leader at once menaced one of the other female goats in the large flock and they clashed horns. Then she wheeled and the others followed her back into the alley. We drove the main flock out to pasture, but the little sub-group of four would not follow the main flock. They could hear them bleat as they went in single file over the hill. We finally eased the orphans out into the pasture and they went about grazing as if they had always been used to this pasture—with one important exception: they took no interest in the bleating of the other goats over the hill. They accepted the three of us who were out taking notes as members of the flock, and I remember two of them were grazing around my feet as I took notes. We could not get near any of the members of the main flock—they would scuttle away; but it seemed from their behaviour that we were members of the little flock.

At the end of the second year—the time that the test was to begin—the sub-group of four had never rejoined the main flock. They did not keep away from them, but they did not mingle with them; it was a neutral situation. Now, at the end of two years, did this early experience of being placed in this waxing and waning, monotonously

anxious waiting situation in the laboratory have any residual effect on their adult behaviour? They were now two years of age.

Four animals, i.e. two pairs of twins, were brought in and confined by a strap against the side of the room. The movements of the foreleg were recorded on a kymograph in an adjoining room. Each orphanage goat was trained against the co-twin that had been with the mother and flock. Each group of four was trained according to a different re-test situation. The first situation was that for a two-hour period in this mass-conditioning unit the now mature goats were subjected to a ten-second darkness signal followed by shock, every six minutes for two hours—twice as long as they were originally tested. Every day, six days a week for twenty-four days four goats—two trained with the mother, two trained in isolation—were set this arduous task of remaining in the laboratory for two hours, waiting six minutes between signals (all ten-second darkness signals).

FREMONT-SMITH:

Were no mothers present this time at all?

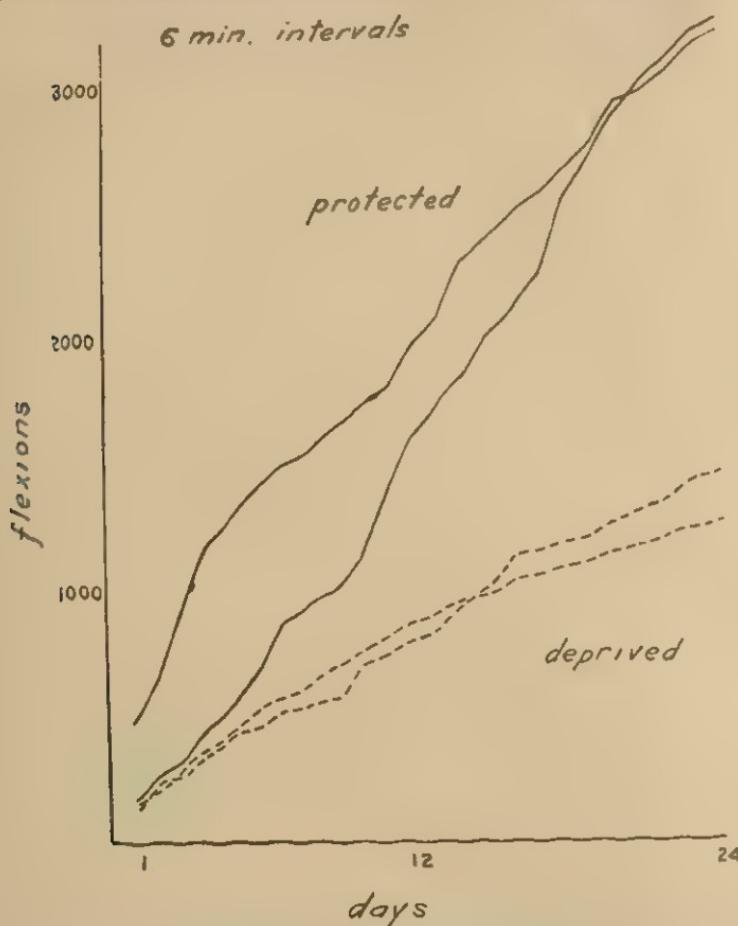
LIDDELL:

There were no mothers present. The goats were all adults and the mother would not be required at this stage. No sheep or goat pays any attention to any other sheep or goat at this age.

Figure 17 is the plot of data from the four goats, the upper two lines from the goats protected during training by the mother and the lower two lines from the ones deprived of a mother's presence. This is a cumulative plot of the number of impatience or 'fretting' flexions of the trained foreleg for each of the four goats when first tested after a two year vacation. The ten-second darkness signal comes on after six minutes have elapsed; then there is another ten-second darkness signal followed by shock, and this sequence continues for two hours.

Two things are to be noticed. In the first place the impatience, or you might say the fretting, flexion movements of the protected animal's foreleg keep occurring. The animals deprived of the parent in training become brow-beaten, or give up. There is another important difference between these pairs. Between the first day and the last day of testing there is a total of about 400 signals. The protected pair missed 25 per cent. of the day's signals on the first day and they continued to miss about 25 per cent. of the signals to the end; that is, they did not get the leg up before the shock. On the other hand, the deprived pair showed steady deterioration in the stability of the conditioned response. They begin as the protected

FIG. 17
ACCUMULATIVE GRAPH OF LEG FLEXION OF FOUR GOATS
DURING INTERVALS BETWEEN SIGNALS 6 MIN. APART



pair does by missing 25 per cent. of the signals at the start, but very soon they are missing more and more signals each day, until by the end of a twenty-four day period, they are failing to react to 45 per cent. of each day's signals. You might say that they are going into a lethargic stage.

FREMONT-SMITH:

It does him no good to make any movement.

LIDDELL:

No, he just gives up.

FREMONT-SMITH:

You call it giving up, but he gains nothing by making that movement.

LIDDELL:

No, but the others who were 'protected' keep on trying.

FREMONT-SMITH:

But still they do not gain anything.

LIDDELL:

No. The whole thing is a fool's game.

GREY WALTER:

As far as the figure goes, it would suggest that the deprived ones are better matched to the situation.

LIDDELL:

In Figure 18 is a graph of the data from four of the other animals—two protected, two unprotected—which were subjected to the two-hourly, daily sessions of ten-second signals every two minutes, rather than every six minutes. Here individual differences between the animals emerge to a more striking extent. This curve seems to indicate that we have two magnitudes of stress to which the two groups of goats were subjected. The two-minute signal interval seems to be the less stressful of the two repetitive time experiments.

MEAD:

The two-minute one is the original?

LIDDELL:

Yes. They all were trained on the two minutes separation of signals.

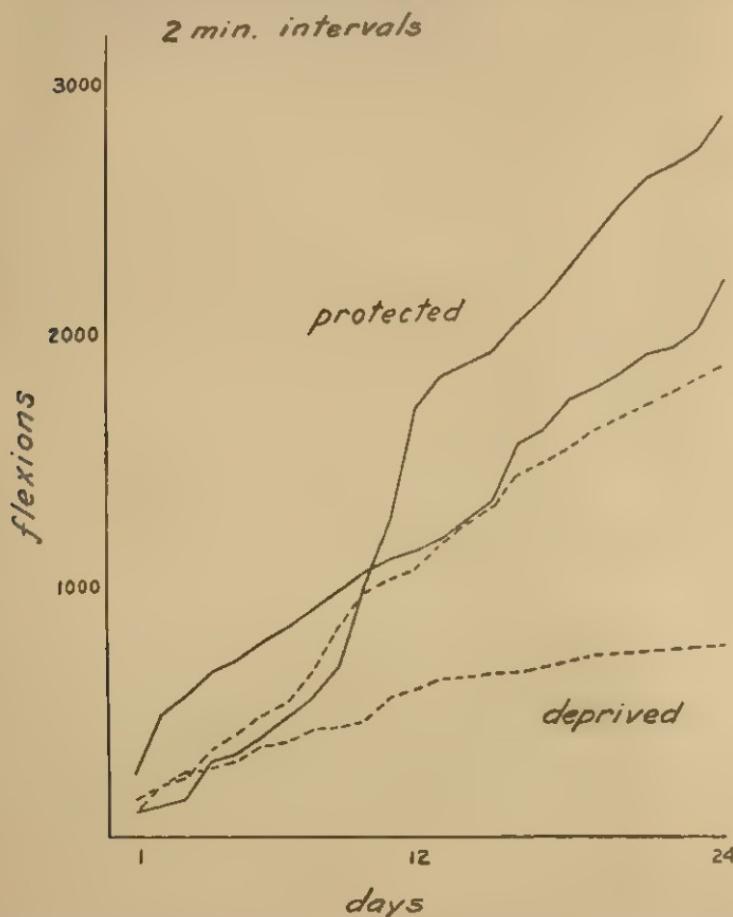
MEAD:

The lesser magnitude of stress is the one they learned on before.

LIDDELL:

For some ten years we have been puzzled by certain facts concerning the neurotic behaviour of our sheep and goats. You get,

FIG. 18
ACCUMULATIVE GRAPH OF LEG FLEXION OF FOUR GOATS
DURING INTERVALS BETWEEN SIGNALS 2 MIN. APART



qualitatively, two different types of full-blown neurosis if you train them to repetitive two-minute intervals or repetitive six-minute intervals.

In the sheep, you get the violent agitation on six-minute separation, but with two minutes separation, the sheep goes into a state of frozen immobility in which both forelegs will stiffen, and he will slide back on his haunches.

Sheep and goat again show a species difference in the initial training of the lambs and kids, when they are subjected to this repetitive signal stimulation with two-minute intervals. The young kid comes to be rigidly immobile and to raise its trained leg stiffly from the

shoulder, and you can get up from your chair and mould him in various positions. He will not resist and he will not escape.

The lamb, however, does quite a different thing. He lies down and puts his chin on the floor, and at the signal, typically, will not make a single ear or head movement. Here he will show absolute indifference to the lights going off. He rolls a little at the shock, and you must roll him over to get the strap off at the end of the experiment. Our animal attendant would come in from the barn and say, 'You have a sick sheep out there', and we would go out and see the half-grown lamb lying on the barn floor, just inert, with the rest of the flock out at pasture. This has happened a number of times; there is a deadening to environment.

BINDRA:

I am wondering about your general interpretation of these findings. What would be the hypothesis, or what sort of conclusions do you hope to draw from this series of investigations?

LIDDELL:

I hope to draw two conclusions. In the first place, when the animal is, let us say, psychically traumatized due to the employment of this monotonous tension pattern, we hope to demonstrate that some residual is left—some 'scar' on the central nervous system which makes itself manifest under stressful training in the adult life. During the stressful training, the one protected by the mother seems to show no residuals of this former stressful training, and we do not know the nature of this protection.

FREMONT-SMITH:

The scar is less if the mother is there?

LIDDELL:

Yes. The psychic scar probably is not there at all.

MEAD:

Did you not also compare the mother-reared twin trained in isolation with the mother-reared twin trained with the flock?

LIDDELL:

I have not got to those pairs yet. We are going to do them, but I will say in anticipation of what we may find that it was very hard

to maintain these separated animals in good health for as long as two years. We found that many of our little lambs—more than the kids—gave up the ghost and died before the first year, at around six months. All our flock are worm-infested. We worm them regularly, but the ones trained in isolation even for this brief time are less viable. We almost decimated our population of those trained in isolation. We have the best veterinary counsel and the diet is regulated by our very competent friends in this field.

MEAD:

And they are with their mothers except in the training period?

LIDDELL:

Yes. I do not wish to labour inconclusive data. Let us go on. We are really coming to a tricky point. I want to see whether Lorenz will agree that we have homologues of displacement behaviour, of imprinting and of innate releasing mechanisms in these mammals.

First, displacement behaviour: Figure 19 (facing p. 65) shows our first attempt at having the animal running free in the laboratory room, with the signal a dimming of lights before shock to the foreleg, wherever in the room the animal might be. This animal, as I remember, is three months of age. In the early training, on the third or fourth day, I suspected that this displacement behaviour might occur and had the photographer present, and the picture shows how the animal passes his waiting intervals between the two-minute signals. He goes into phantom-grazing movements. These are real grazing movements. Years ago in the maze, when the animal was first learning it, he would annoy me no end by starting down the central alley, suddenly stopping and beginning to crop at imaginary grass. I would have to get down on my hands and knees and weed the maze every day, and still the sheep would find an 'imaginary' blade of grass and take his time about it. He did this phantom grazing on the bare cement floor.

Another trick the sheep likes to play is to give up and take a rest. Sheep have a sinking-down reaction—if you chase them in the pasture they will sink down and give up. A sheep in the laboratory room would lie down, and very often would get up just before the lights went dim and start walking very rapidly towards the corner where he preferred to be when he got the shock.

BINDRA:

What is the stimulation for this?

LIDDELL:

Nothing: two minutes wait between signals; signals for ten seconds.

BINDRA:

No shock?

LIDDELL:

No. This is behaviour just to pass the time, to relieve the tension.

Now, the goat has a different type of displacement behaviour. He likes to rear up on his hind legs. We used to see them doing that in the maze. They would go along, then suddenly rear up and look over the fence. There was nothing to see. They would sometimes do a little grazing too.

Now the next point. We trained a normal ram three months of age; it learnt to flex the leg at one rate of the metronome, and to leave it down at another rate; it grew and thrived until it was three years of age. It was anatomically a normal ram, but it never impregnated any ewes, it did not show any male aggression, nor did it vocalize after training was well begun. When it died we were without a demonstration animal, and we secured a normal ram, one year of age, fully mature. We put him in the same box that we use for our goat demonstration, and then sounded a buzzer signal for ten seconds, gave him a shock, waited a minute, sounded the buzzer signal again, and so on for forty signals; all he did in response to his first training situation was to show male aggression and, instead of flexing the foreleg at the signal, he began pawing and butting movements. Later a little trait of leg flexion would appear now and again, but mostly masked by pawing and butting.

Then, he gave a menacing bellow, and if you approached him he would bellow at you and try to butt you; he would not wait for his signal, but started butting and bellowing and pawing in the interval between the signals. All we got out of him was male aggression. Then we tried this same procedure on another adult ram, and after a certain point he gave up his male aggressive behaviour and became rigidly immobile, and we never got anything out of him except muscular stiffening, in spite of repeated signals and shocks. So then we have started two little rams on a similar training schedule. Soon butting and pawing feebly emerged; now this male fighting reaction has gone underground in both little rams.

We may here be on the trail of something homologous to the process of repression in psychodynamics; repression is not the same

as this homologue at a primitive level, where we have an innate behaviour mechanism being forced underground by habituation.

LORENZ:

I wonder whether you ever had some of your experimental animals on the bare floor of the laboratory as controls without any stimulation. I mean without any stimulation that elicits grazing; of all the activities described, the grazing is the most certain *not* to be autochthonous. You see, the rearing and looking about, though it might be a displacement activity, might also be an autochthonous rearing up and looking for a way out, and so might the aggressive behaviour of the ram simply be autochthonous. But grazing is indubitably a displacement activity. My arguments about the other behaviour patterns are merely those of the *advocatus diaboli*: I really do believe they are displacement activities. But as regards the grazing, I am sure of it. Besides this would be easy to prove; but you need one control experiment to exclude autochthonous grazing; you put a sheep in the same situation on a bare floor without any stress situation.

LIDDELL:

Yes, we did, and got no grazing.

LORENZ:

That seems convincing.

LIDDELL:

We even went this far: we thought perhaps darkness in the sheep and goat might be the releasing stimulus.

LORENZ:

To grazing?

LIDDELL:

For some type of behaviour, because the sheep will congregate together in darkness. But we turned the lights on and off ten seconds every two minutes and observed the animal; it did nothing, it wandered round the room and paid no attention to the lights after the first time or two.

BINDRA:

There is an experiment on rats by ULLMAN (1951) that is probably relevant here. These rats were shocked periodically in a situation in which they normally ate. After a few trials, Ullman observed this: just as the time of the shock approached, the rats began to eat at a very fast rate, and kept on eating until the shock was over. Then they sat back, and when the time for the next shock came around, they began eating at a very fast rate again. But in that case the food was there all the time. I should think that *displaced grazing* implies that the animal has probably some sort of an image, he 'eats' what he imagines to be there.

LIDDELL:

In our first conditioning experiments we performed the reverse of this experiment, and confirmed it a number of times, though we have not systematically pursued it. The sheep was confined by the conventional Pavlov frame with a bucket of oats placed in front of him; he reacts to the clicking of the metronome by flexing the leg, he gets the shock and at once plunges his muzzle into the oats bucket and really eats. This is quite stereotyped and we have two or three movie sequences showing it. We did not know what to do with it theoretically, this forced feeding reaction, so we just recorded it and let it go at that.

BINDRA:

The grazing response is a very strong response. When they are out in the pasture, are they not grazing all the time?

LORENZ:

Much of the time.

BINDRA:

Not like rats that eat every two or three hours, and only for about ten minutes at a time.

LORENZ:

This is perhaps important. If an activity is normally performed all day long, then the endogenous production of this activity is of course phylogenetically adapted to this enormous demand; and these activities are the ones that are the first to appear as overflow activities, as we prefer to call them. I can give you a good example of this, the pecking movement in different gallinaceous birds. There is one

partridge-like bird—the cacabis, I do not know what it is called in English—which lives in Southern Europe, in stony parts where the bird has to peck all day long to get its fill. And in captivity this species does more overflow pecking than any other gallinaceous bird I know. A similar rule holds true for displacement activities also. The behaviour patterns which we find most frequently as displacements are mostly those which are, so to speak, cheap, produced in great quantities to supply huge demands. That is why the skin-comfort activities are so awfully frequent as displacement activities. And if you ask me what would be the instinctive activity which I would first expect to appear in a sheep as a displacement in a situation of stress, I should have said: grazing—and it would be gnawing in a rat.

LIDDELL:

And in the goat?

LORENZ:

We'll, it is difficult to say.

LIDDELL:

I think it would be rearing.

LORENZ:

It might be.

LIDDELL:

I think we must have Dr. Lorenz specify for us where his notion of displacement behaviour may apply to our mammals and where something, perhaps more plasticity, must be added. Displacement activities are innate activities, are they not, not activities built up by training?

WHITING:

I think I am confused out of ignorance. I should like someone to clarify for me what displacement means in the ethological sense.

LORENZ:

It has nothing whatever to do with the psychoanalytical term displacement. This point was brought up in the Cambridge Conference and there psychoanalysts said there was no objection to calling these phenomena '*displacement-activities*', because the psycho-

analytical term 'displacement' is always used by itself and there was no danger of confusing it with displacement-activities. As for the displacement-activity, we do not know what happens physiologically when it occurs; but all we know is that when an instinctive activity is released and then re-blocked by the stimulation evoking another, incompatible activity, the animal shows movements belonging to neither of the two conflicting drives but, surprisingly, to those pertaining to an altogether different instinctive activity. We do not know what this type of displacement really is physiologically, but paradoxically we know something about where it happens. It must happen between the central mechanism which releases an instinctive activity and the peripheral motor elements activated by the latter. Besides conflict, there are some other types of situation also evoking displacement-activities, but they all have in common that the already activated instinct is prevented from finding its adequate outlet in the motor patterns normally pertaining to it. In most known cases true displacement-activities are aroused by conflicting drives. The classical and most frequent example is that of aggressive drive blocked by escape reactions. It is very characteristic of displacement-activities that, in this conflict of two given drives, the displacement-activities which appear are always the same. When aggression is blocked by fear in a greater snow goose, there invariably appears a bathing movement. In a pink-footed goose exactly the same situation that will evoke bathing movements in the snow goose elicits a queer sideways distortion of the neck. Only when the conflict gets higher and higher this movement becomes recognizable as the behaviour pattern which, in its autochthonous form, is used for the distribution of fat from the oil gland in the swimming feathers by all Anatidae. So different are displacement activities in species so closely allied to each other that they would give fertile hybrids. This example shows the specificity of the *direction* in which this displacement occurs and also the absolute species-predictability of these phenomena. Dr. Liddell always gets grazing in one typical situation in different types of sheep and he always gets rearing in different types of goat. Displacements go in amazingly different directions in different species. They may be the strangest thing you can imagine: an avocet threatening a rival male suddenly makes the movement of sleeping, putting his head behind the wing and glaring at his antagonist all the time.

TANNER:

In your two species of goose with the different displacement-activities, are these the ones *always* produced, whatever the two conflicting instincts?

LORENZ:

No. Every combination of conflicting instincts may produce its own displacement-activity. A mallard will preen if a slight sexual drive is activated by his seeing a female, while he does not yet want to touch her. If his aggressive drive is blocked by fear he will drink. If both situations are combined he will preen-drink. If he drinks, you will know he wanted to be aggressive and did not dare to. If he preens you will know he wanted to importune the female and did not dare to. So, the displacement-activity is quite specific for one conflict.

TANNER:

And they are all the same within the given species?

LORENZ:

All the same within the given species. Once you have analysed the given situation, you know exactly which drive is conflicting with which by watching the displacement.

GREY WALTER:

Which way round should one use this information as to mammals?

LORENZ:

We know so confoundedly little about mammals.

GREY WALTER:

In your bird species you have information about instinctive behaviour, but in the mammal we seem to know so little, I do not feel quite sure which way round we should start. Should we say: Displacement-activity in the dog or cat or goat or man is so and so and when this happens this is a displacement-activity; or should we start the other way round and say, such and such is a constant thing, therefore it must be an instinctive activity?

LORENZ:

It might be displacement-activity in a given situation. But if you have not analysed what the situation is, and just find a given situation always elicits a given activity it might just as well be autochthonous activity which has been released by adequate stimulation. But the hamster, about the best known of our experimental animals, shows quite a number of very characteristic displacement-activities. When

the male hamster pursues the female and loses her suddenly, he always proceeds to preen, to lick himself; and if he wants to be aggressive and does not dare to, he will gnash his teeth. This activity in its autochthonous form is probably used to sharpen the teeth.

FREMONT-SMITH:

Could one see a utility in the displacement in two senses: for instance, preening could conceivably make him more desirable to the female?

LORENZ:

I think that this is far-fetched; no.

FREMONT-SMITH:

Does preening precede sexual activity normally?

LORENZ:

No. But it has a certain survival value, which was emphasized first by TINBERGEN (1940): it may have a cathartic function by working off the blocked drive, by opening a safety valve, so to speak. The major new finding in ethology since our last Congress was by Piet Svenster, a pupil of Jan van Iersel in Leyden, who actually measured the cathartic function of displacement-activity in the three-spined stickleback. He found that frustrated sexual drive in the stickleback produces a parental function in displacement, the movement of fanning the eggs. He could make this animal fan by taking away the female and blocking the sexual drive; then in some animals, he facilitated the fanning by providing adequate stimulation for autochthonous fanning by supplying a source of CO₂. (The CO₂ emanating from the eggs provides the autochthonous stimulus for airing them by fanning.) By this procedure, Svenster got, so to speak, a mixed fanning: displacement fanning slightly facilitated by supplying autochthonous stimuli. Then, by showing dummy females and recording the intensity of the response, Svenster measured the male's readiness for sexual activity. He found that, by facilitating the outflow of the displacement-activity he had lowered the internal pressure of sexual activity. There are still some objections to his paper, but I do not think they are correct and I think they will be removed by further experiments. Svenster was the first to show that it was really the frustrated autochthonous drive which came out in displacement-activity, and that there was a cathartic function, a safety-valve function in it.

FREMONT-SMITH:

It leads to a real discharge of energy?

LORENZ:

Yes.

BOWLBY:

In psychoanalytic 'displacement' the energy is discharged by an act which is consistent with the original drive. For instance, if you feel aggressive with *X* and you hit *Y*, you feel better for it. You relieve your pressure by an act of the same order as the original drive, but to a different object. The existence of psychoanalytic displacement is commonplace. But I believe that displacement-activity, speaking ethologically, also occurs in human beings. I suspect that a great deal of thumb-sucking is displacement-activity; masturbation is likely to be the same.

BINDRA:

It is all right to call these activities displacement-activities, so long as these activities are defined in strictly behavioural terms. What I think Dr. Lorenz has done is to define these activities in terms of certain causal agents: an activity is a displacement-activity when it is brought about by conflict of certain instincts, or an activity is a displacement-activity when the animal is anxious in that situation. I think we have to be very careful and make sure that the activity under discussion is defined strictly without any reference to the causes. It is only when we define behavioural categories strictly in terms of actual happenings, acts, without any reference to causes, that we can legitimately ask the question: 'What are the causes that give rise to that particular activity?' But if we *define* displacement-activity as activity which results from certain types of instincts, it becomes logically meaningless to ask what other conditions produce this same activity. Instead of making 'displacement-activities' a category of behaviour, we should only use grazing, leg flexion, etc., as behavioural categories. Then we can state or argue about the conditions under which that particular type of behaviour, grazing, for instance, is manifested. And then we can state how anxiety, or conflict, can or cannot produce this activity. But using the term 'displacement-activity' is simply begging the question.

LORENZ:

But we do know positively one thing: that in order to get the pink-footed gander to preen, I have first to make him furious by

putting a family of greylags before him, withholding the gander of the greylag family, and then he will start threatening and attacking, not thinking of displacement-activity; then I loose the father of the greylags, of which the pink-footed gander is afraid, and then he would do displacement preening. The point is: I must remove the greylag gander first, or else the pink-footed gander would not attack and, if I do not let the greylag gander loose, he will just go on attacking with his neck stretched forward and he will not 'displacement-preen', because the geese without the father will just escape from him without causing him any conflict. In a case like that we know perfectly well which instincts conflict with which.

But you are quite right about our terminology; it ought never to imply that we know the causal explanation; that is exactly why we relinquished the term 'sparking-over activity' which is very descriptive, but which implies a lot.

Displacement means just that the energy which we know to exist, comes out in another way from that in which we expect it to come out.

GREY WALTER:

Energy in what sense?

LORENZ:

Specific readiness to a certain activity.

BINDRA:

Activity appears where it is least expected in that situation.

MEAD:

Irrelevant activity?

GREY WALTER:

Before you can say that a thing is irrelevant, you must know what else could happen, must you not? What are the alternatives?

FREMONT-SMITH:

The goose could do many other things.

GREY WALTER:

How many?

LORENZ:

He could fly away.

GREY WALTER:

How many are there, actually?

LORENZ:

Roughly two hundred and fifty.

GREY WALTER:

Two hundred and fifty alternatives, of which the goose's preening is one, and it is always the same reaction? In all these matters we should have some estimate of the alternatives, otherwise we have no information at all.

LORENZ:

In preening alone he would have about ten quite separate instinctive movements, which are not intensity gradations of one pattern. Or he might do any of his expression movements; or he might simply flap his wings.

GREY WALTER:

These things are known to occur as displacement-activities?

LORENZ:

Yes, in other situations.

GREY WALTER:

How many of the two hundred and fifty behaviour modes are known to occur as displacement-activities?

LORENZ:

At least ten or twelve in this species.

GREY WALTER:

If one applies this to the mammal—do we know anything about the number of modes of behaviour of the sheep?

LIDDELL:

They are relatively few.

GREY WALTER:

Much less than two hundred and fifty?

LORENZ:

Much less.

GREY WALTER:

I have never seen a sheep doing anything but grazing!

BINDRA:

You have hit the nail on the head. However, I do not think that the number of modes of behaviour as such is the relevant variable; rather it is the activity or mode with the highest probability of occurrence. That is to say, if you took time-samples of the animal's behaviour, what is the animal doing most of the time? Sheep you see grazing; birds preening; and so on.

HARGREAVES:

Are they always the commonest current activities, or may they be reverersions to old common activities? For instance, sucking in cats. I have the impression that early weaned cats as adults develop nostalgic sucking of cushions and things at the time that they have given up sucking. In the adult it may not be habitual, it may only be a temporary reaction. Perhaps the displacement activity, in a mammal at any rate, could be an archaic habitual action.

RÉMOND:

A tic is an activity which arises under some sort of stress and which takes the place of something else. Is this related to displacement-activity?

BOWLBY:

It seems to me a very plausible hypothesis on which to work. I doubt if there is any particular evidence about it, simply because no one has thought about tics in this way before, but it does appear that these kinds of movement occur when other activities are frustrated.

I think that tics can probably be explained in two quite separate ways. One type of tic is probably a form of displacement-activity. The other type is similar in form to an intention movement. (An intention movement is the very restricted brief beginning of a full innate behaviour pattern.) Anna Freud has described (Freud and Burlingham, 1943) children who were going through what appeared to be a completely meaningless repetitive movement and have been able to trace these movements to their origin in a meaningful movement, which is a fully developed pattern of behaviour. In the case

that Anna Freud quotes, the brief movement of this kind was the remnant of a full behaviour pattern which had as its content: 'soon, my mother will come and she will bring my coat and my hat and my shoes and she will put them all on'—and he went through the whole rigmarole of putting them on—'and then she will take me away'. After some weeks and months of this it had become an apparently meaningless fragment of a movement. Robertson has also observed something quite analogous (Bowlby, Robertson and Rosenbluth, 1952).

These two hypotheses regarding tics are not in any sense contradictory; it would not surprise me if there were two sorts of tic. Tics are highly compressed, and any one may have more than one root.

MONNIER:

Tics are very often the mere expression of an overflow, something comparable to the tail twitching of the young lambs in Dr. Liddell's experiments or to observations we made in babies with early disintegrated brains. We know from cases of toxoplasmic and rubeolic embryopathy—that is cases in which these diseases occur during pregnancy and injure the brain of the foetus between the third and the seventh month—that these children show later an increased tendency to tics. Those are certainly primitive motor patterns which would be adequately controlled in a normal brain but which appear to be released when the cortical brakes have been destroyed by some pathological process. My impression of tics is that they belong also usually to this category, of released motor patterns in disintegrated brains.

TANNER:

I do not see how the tic could really be a displacement-activity, in that it is not in itself a fully developed movement which does something else normally. It must be a very fragmented displacement-activity, if one at all.

I want to ask Dr. Lorenz whether there are actually any known instances where a particular displacement-activity is characteristic of the young of the species but disappears in the adult; or is displacement-activity always an adult activity associated normally with another instinct?

LORENZ:

Well, I simply do not know, because there are a lot of phenomena which we do not quite like to include in our concept of displacement-activities. There are stereotyped, acquired movements, not so much

learnt as ground in, in the way described by Dr. Grey Walter. These may appear in the same way as displacement-activities do. This also occurs in captive animals, mainly because only captive animals have such deeply ground in, stereotyped movements, such as caged parrots doing their typical bowing, which was originally an intention movement of flying away. Even man will show displacement-activities, only his variability is incomparably greater. One man will always tighten his tie and another will flick his ear, but you will find a strong preponderance of skin comfort activities. The tic which originally gave rise to the word is a skin-twitching activity often accompanied by the movement of the hand. I think the genesis of such movement in humans is very complicated. The motor pattern underlying a tic may have originally been an intention movement, but may have turned from an intention movement into a stereotype, and from stereotype it has developed into a displacement-activity.

A more important question is whether infantile movements may reappear. MONICA MEYER-HOLZAPFEL (1938) has shown that bears and monkeys and some other mammals do activities of thumb-sucking in moments of stress. I think I noticed in domestic cats the same thing which Dr. Hargreaves mentioned. I doubt if these are really displacements. They might also be regressions due to disintegration taking place in situations of stress. This might look very much like a displacement, because it appears in similar situations, yet it might be caused in an entirely different way.

GREY WALTER:

Weeping is common in children and relatively rare in adults, and is a characteristic of stress—or does that make it absurd?

TANNER:

I want to ask Dr. Lorenz something again for clarification, since you suggest that weeping might by stretching the terminology be considered possibly as an example of displacement-activity in a certain sense. It strikes me that when somebody weeps you, another human, know what that means. Now, when a goose does displacement preening, what do the other geese know about its feelings, or does it not tell the geese anything at all?

LORENZ:

Well, you have put your finger on it, because we do not know one instance where a true displacement which is not ritualized is understood by the conspecific. In other words, wherever we find a specific I.R.M. responding to a displacement-activity, there already

is ritualization. This may be explained by the simple fact that once there is an I.R.M., it selects very rapidly in favour of the sign value of the displacement-activity, and would make it more semantic. The I.R.M. will 'breed' the activity in the direction of semantics. The same process of exaggerating semantic values also occurs with the intention movements. Wherever intention movements are 'understood' by the fellow member of the species they are very apt to be already highly ritualized and distinguishable from the non-semantic homologous intention movements. We know only one example of one expression movement where there is quite clearly an I.R.M. and no ritualization at all, and that is the human yawn whose contagious properties are proverbial. It is exactly the same yawn as in non-social mammals, where it effects no social induction at all. A jaguar or a wild cat, or whatever you want, yawns in exactly the same way as man.

MEAD :

I just want to be sure about what you said. These true displacement movements are not unintelligible to other members of the species. Is the definition of irrelevance applicable?

LORENZ :

Not always. They just happen to be that way because we never caught them in the moment where it was 'already' understood and 'not yet' ritualized, you see. Once the species develops an I.R.M., the thing becomes ritualized so quickly that we have never happened to catch one in the moment where there was already an I.R.M. corresponding to it, and not yet any ritualization.

TANNER :

This is how instinctive movements evolve?

LORENZ :

This is one instance in which we believe we know how they evolve, and that is why we think it is so important: it is one instance where we think we can show causation in the development of instinctive movement.

MEAD :

In this goose that preens when it is interrupted between fear and aggression, the other geese respond to that preening: is that right?

LORENZ:

No, not yet. Maybe they do—but it has not yet been proved experimentally.

TANNER:

I think that Dr. Lorenz's point is that later on, after a long time, some goose may discover the significance of displacement preening, and then this will have a selective value, a new I.R.M., and then you have got two hundred and fifty-one movements.

BINDRA:

I should like to state two different interpretations of the so-called displacement phenomenon. I think Professor Lorenz's interpretation is essentially this: whenever there are two instinctual tendencies in conflict there is discharge of the energy or excitation connected with these instincts into a third channel which may be preening or grazing or some other such activity.

My alternative interpretation is this: that, let us say, there are two instinctual tendencies, which when excited together, lead to a general emotional response, a general excitement. This general arousal leads to a release of those activities which happen to be the most prominent in the repertoire of the animal.

LORENZ:

No, they need not invariably be the most prominent. Occasionally you get very rare activity too.

BINDRA:

I think difficulties of this kind can be taken care of by postulating that different thresholds or excitations are required for different types of activities, so that as the animal reaches different levels of arousal certain responses become more likely to occur.

LORENZ:

I want to emphasize that we have no interpretation at all. We just see that in one specific conflict situation one specific displacement-activity occurs. For brevity's sake, I have hitherto only mentioned conflict, but there are other situations where displacement-activities habitually occur, for instance when the specific consummatory situation of some activity is reached too quickly, so that some general excitation is 'left over'. Then there is one special type of

displacement-activity which we call 'laziness displacement', and that is a very curious thing. If the specific level of excitation *just fails* to reach the threshold of an activity, you may get a displacement. For instance, if a cat has some incitement to go in a certain direction and then decides not to after all, it invariably preens its neck.

FREMONT-SMITH:

Sort of pretending it never intended to anyway, you mean?

LORENZ:

Yes. Heinroth describes how, when mallard drakes are invited by their females to copulate, they always 'invent an excuse'; that is to say, the drake goes through a few initial movements of copulation and then starts preening. Also, if a mallard is swimming towards a bank and wants to go up that bank because there is some food on it, but it is not hungry, it invariably swims up to the bank and then shakes its tail. This is so specific, and the general level of excitation is so very low, that I would not like to assume that this conflict of wanting to go up to the bank but being too lazy to do it after all causes such a high level of excitation that it must 'come out sideways', so to speak. I think this assumption is not quite compatible with the laws of parsimony of thought. There is nothing indicating a very high general excitation.

Then maybe I ought to emphasize one thing which may even be in favour of Dr. Bindra's interpretation, or it may not, that is the immense drop of potential between the original excitation and the intensity of the displacement-activity. You have to get a stickleback male into a glowing rage and, at the same time, make him desperately afraid, to get just a little bit of displacement digging, which is so low in intensity that Tinbergen at first mistook it for eating. Only by putting a particular strong motivation and inhibition on the stickleback by crowding the tank did Tinbergen succeed in eliciting displacement-activity of full intensity. Then he got all the normal sequence of movements of digging and nest building. In very high states of conflict he even got the glueing activity (the stickleback glues the fibres of the nest together with a special secretion from his kidneys). This activity represents the highest level of excitation of all nest-building activities. This shows that Dr. Bindra's principle of threshold differences may be applied very directly to what I am saying just now. In the case of all these nest-building activities of sticklebacks, activated by displacement, it is quite evident that the digging movements have a lower threshold than the others, while glueing has a higher one. This difference of threshold is quite the same

whether the movement be elicited autochthonously or by displacement. In both cases, a lesser degree of excitation is necessary to elicit digging than to elicit glueing. This makes it very probable that the displacement does not influence just one movement, like digging or glueing, but a higher centre which, within the central nervous system, controls both of them. Of course the explanation, that different thresholds determine which activity shall occur, is only applicable within the narrow limits of one specific readiness to certain activities, like nest building. There is no such thing as a 'general excitation', and saying, for instance, that the aggressive drive has a lower threshold than the sexual drive, is in principle nonsensical.

MEAD :

Is it possible that the social irrelevancy of these acts is itself a clue? The non-communicability, the absence of an I.R.M., itself might be significant. When the animal or bird has got into a situation of high social intensity also involving conflict, which interferes with its carrying out the intent to flee or copulate or whatever it is, the assumption of some piece of behaviour with which it can at the same time occupy itself might have survival value.

TANNER :

Going back to the cat, is it not possible that this displacement-activity is one way of saving yourself from remembering everything? If the cat is going off in one direction and then decides that he does not want to go off in that direction after all, it is better from the survival point of view for the cat to wipe the false start right out because he may next time want to go in a different direction. We know some traces persist for some time and if there is no wash-out the animal may start off in the resultant of the new and the old directions, which would be very unfortunate and allow the other cat to get him on the blind side. It would be one way of wiping the slate completely clean, would it not? And it is a method he could use without getting involved with other members of his species.

LORENZ :

That is a finalistic explanation. Yet this aspect of a new survival value is very interesting.

MEAD :

It would have survival value to this individual for future action and also the advantage of *non-communication* of intention to other members of the group.

LORENZ:

Would you say that it is an argument against your explanation that displacement activities *do* so very easily and so very often develop into means of communication?

MEAD:

No, because anything can develop, and as long as you *define* your displacement activity as one which is not ritualized and which does not have semantic content, the explanation could hold.

LORENZ:

Of course, ritualization means the creation of a new instinctive movement, of a new motor co-ordination which copies itself and superimposes itself on the displacement. Once you have that superimposed independent co-ordination, you have no more displacement-activity in the physiological sense. The trouble is that evidently—we know it by comparative studies—this superimposed, new, instinctive motor co-ordination evolves exceedingly quickly once the stimuli emanating from the displacement-activity are taken up by an I.R.M.

FREMONT-SMITH:

I think I have an example, which you all know, which fits in with Margaret Mead's concept; that is what happens when two kittens are at play and attacking each other fiercely with their ears flattened and so forth. All of a sudden one of them, as he sees the other one is about to attack, will turn away and walk off, obviously ignoring the situation and having no intention of doing anything at all, as if he is interested in something else. Then all of a sudden he swings around again and leaps upon the other one. This seems to me to have a survival value, and it is part of what you were talking about.

LORENZ:

I am afraid there is a different explanation. I was just about to mention play as a phenomenon relevant to Dr. Bindra's theory. In play, you find something very parallel to displacement-activities, because in play, as in displacement-activities, there is a consummatory act, that is, a low-level instinctive motor pattern elicited by something other than its normal specific sort of excitation. This accounts for the fact that in play, as in fits, you may get instinctive activities pertaining to quite different instincts in a quite irregular,

chaotic, sequence. In play, and in no other situation, you get the movements of fighting and hunting in one sequence. A kitten will stalk another like a prey, then rush at it, stop before it, claw it, do some real fighting movements, rush away, then stand in defensive attitude, with its back up. So in three consecutive seconds this kitten will treat another (*a*) as a mouse, (*b*) as a rival tom cat, (*c*) as a dog, and then switch off and just do nothing.

In Masserman's and Hess's experiments (Hess, 1949) you get something very similar. If you stimulate one place in the hypothalamus, you evidently get the whole instinct away from the highest level which, in Tinbergen's hierarchy, is the highest centre; you get a fighting mood in the cat, and the cat will be less responsive to being fondled; it will refuse to purr and it will then attack some real object. You get all the fighting patterns with the exclusion of other patterns. If you remove your electrode from this spot just a few millimetres caudally, you will get the same movements but dissociated from each other. You will find you can still make that cat purr or eat and it will not be in a fighting mood at all, but you may quite suddenly elicit one lower level activity of fighting, a dissociated blow of the paw or just one spit. That is what was called sham activity originally, sham rage as opposed to real rage. In confirmation of what Dr. Bindra says, in both cases there is some non-specific excitement which is able to circumvent the higher centres and elicit independent, uncorrelated, lower level motor patterns which is something very suggestive and very interesting.

LIDDELL:

Suggestive of what?

LORENZ:

Suggestive of the fact that instinctive activities must be very firmly fixed patterns, as they can be activated in quite another way than the one in which they develop naturally and yet show the same pattern of complicated motor co-ordination.

MONNIER:

I should like to emphasize another aspect of these experiments of Hess (1949). He was able, as you know, to lower the threshold of various instinctive or motor patterns, which showed very interesting plasticity responses according to the actual situation. For instance, when he induced the mechanism of head turning to the right, it occurred frequently that the cat kept this position and started to lick

its shoulder. It behaved as if it was trying to adapt the forced posture to the situation, thus giving the impression of some kind of cortical adaptation. I can give you another example of such instinctive patterns secondarily adapted to the situation. For instance, stimulating the hypothalamus you may increase the readiness to activity of the animal. The animal stands on the table with two possibilities: it may fly, or it may attack. If the animal is surrounded by observers and cannot escape easily, it will attack one of the observers. In the other case, if the animal sees a possibility of escaping, it will do it. This confirms the plasticity of the electrically-induced reactions.

LORENZ:

That depends on whether you are stimulating higher up or lower down in the hierarchy.

MONNIER:

I meant a stimulation of the hypothalamus.

LORENZ:

I will give you another instance which Professor Hess himself showed me in a film. He stimulates the centre for head lowering, which is an intention movement downwards. The cat is standing in the middle of a large table and it starts putting down its head. Then it runs to the edge of the table in order to be able to put its head further down than the level of the table. That is, of course, very suggestive of an irradiation to higher centres.

Perhaps I can illustrate what I mean by telling you of a joke. Erich von Holst and I talked about the body/mind problem and asked ourselves what would happen if Hess stimulated my brain centres, my attack centres. I would suddenly remember that Hess had misquoted me in one paper of his and has treated me rather badly in some respects, and maybe I did not like his beard or something. Then I would be quite convinced that I had very sufficient reason for attacking him.

That is only a rather jocular parallel. But before leaving the subject of displacement-activity I would call your attention to a statistical, though not yet certain, correlation between posture and displacement-activities. It seems that blocked, frustrated drive very often finds its way into those activities which are already prepared by the animal's present attitude. For instance, there is a correlation between the intention movements of ducks for flying away and the displacement head-shaking which will take place in this situation. There are

some *Anatidae* which show a primitive form of aiming movements, like other birds. Then there are some which only move their heads vertically up and down, like the mallard, and there are some, like diving ducks and geese, who only give a short upward flick of their bill. All these are intention movements, not displacement-activities. In all these species there appears a displacement movement of a sideways head shaking. This never occurs in the species which move their heads in the original way; most of them occur in species which just do this flicking. The movements which are already prepared by the attitude very often appear as displacement-activity, and this licking Professor Monnier mentioned may be a displacement induced in this way.

RÉMOND:

May I take the opportunity to put a question which will take us into a very different subject. I would ask Dr. Liddell how he would define in all these presentations the word or the entity of stress.

LIDDELL:

From my present point of view, stress is to be defined in terms of the organism's preparation to neutralize the stress, and I have tried to define it from Cannon's original description of an emergency reaction in which, in order to maintain homeostasis, physiological patterning must occur to bring about those adjustments necessary to supply the brain, the heart and the skeletal muscles with the materials required for the anticipated exertion, and of course in Cannon's extreme case the cat must fight or flee for its life.

My present conception is that Pavlov's classical conditioning method is a means by which we may greatly refine the quantitative gradations of this emergency reaction in terms of precise stimuli which the experimenter may control, and so it is my present belief that the intensity of stress is to be correlated with the intensity of the bodily processes' anticipation for exertion. When the animal comes into the familiar experimental room where conditioning has occurred, it begins its preparation to meet an emergency, a potentially dangerous situation. We can show, when the animal is standing there awaiting the expected signals for action, for meeting emergency, its breathing is modified, the heart-rate is increased, the psychogalvanic response can be observed. Then the specific conditioning signal, either positive or negative, sharply increases the animal's physiological re-patterning to meet the crisis which is now about to occur. Then when the signal—a neutral stimulus—is given, that itself arouses an alerting response which I find indistinguishable from

Cannon's emergency reaction. It is found that the more intensely attention-getting this neutral stimulus is, the easier is the coupling established with the reinforcement. The animal is already in a state of tense expectation, it is in a situation in which it is psychologically trapped—it has given up attempts to escape—and the conditioned signal arouses emergency action which increases in intensity up to the moment of reinforcement. Suppose this perfunctory and mild electric shock forces the animal to make a defensive movement, then the stress is relaxed. So if you are going to speak as Selye does in terms of stressers, there must be relaxers. But when the conditioned stimulus has been given, the reinforcement follows. An episode of intense emergency preparation has now been terminated by the forced relaxation. But this intense preparation decays more and more slowly, and then the next signal ushers in another period of intense emergency reaction, and then (suppose that this is a negative condition signal), the animal is tensed and prepared; the heart beats faster and breathing is more laboured. You can visibly see the goat puff and pant at the bell which means no shock. It is obviously reacting at a higher intensity, but no reinforcement follows; there is no relaxer to follow this new stresser. Therefore, the animal's emergency reaction to the negative condition signal has no drainage, no means of quickly waning.

LORENZ:

Now I understand why you are always changing between a meaningful stimulus and a negative stimulus. It is only now I understand what the positive and negative stimuli are!

LIDDELL:

Then when the animal is ushered out of the experimental room, he is not at once completely relieved of stress, because there is a radiation effect. For example, a neurotic stiff-legged goat is now released from the Pavlov harness, but his leg remains stiff and he limps out of the room. As soon as he goes over the laboratory threshold, the leg loosens up and he can run.

I therefore say that, to me, stress is judged in terms of intensity of reaction. Special conditioned stimuli are stressers and reinforcements are relaxers. From day to day the animal is introduced into a situation which, through habit which is itself stressful, has prepared him to meet emergency. Minor emergencies are ushered in as signals, and the negative conditioned stimulus, in my experience, is more stressful than the positive, because there is no forced relaxation at the end, and very often we have got experimental neurosis from

repeating the negative signals over and over because there is nothing to let the animal down or to lower the intensity of its emergency preparation.

LORENZ:

He would like to be rid of the expectation?

LIDDELL:

That is right.

RÉMOND:

You speak about a stress having occurred. You have to have reactions to that stress. If there were no reactions there would have been no stress?

LIDDELL:

To me as a biologist studying behaviour the term stress can only be measured in terms of the animal's reaction thereto, and what is a stress stimulus for one organism may not be for another. I will give you specific examples. One of my experimenters spent the whole year with four goats trying to build up the maximum delayed conditioned reflex, and he began with a telegraph sounder clicking once a second and on the sixth click the animal got the shock. So very soon, the goat began to lift his leg in preparation for the shock say at the fourth or fifth click. Then clicking continued for ten seconds, then he would delay ten to fifteen and twenty. Finally, the best goat had a very well established hundred-second delayed conditioned reflex, which is the maximum we have obtained in any of our goats. Two of them became experimentally neurotic, but this one succeeded in mastering the required delay. We employed a cardiotachometer to correlate the degree of stress with the heart-rate. The metronome clicked, the heart was accelerated, then slowed, then was accelerated again by the next click and so on, so that it continued to accelerate along a saw-tooth type of curve. In the hundredth second, when the animal got the shock, the heart-rate went back within two seconds to the pre-signal level, so sudden was the relaxing effect of this shock. On the other hand, the same goat, standing in the Pavlov frame with intense expectation, received an unsignalled shock. The heart raced instantly and took several seconds to slow down; the same intensity of shock administered to the same animal. The stress under one situation was tremendous before the shock relieved it and the other was minor because it was over as soon as it had appeared.

LORENZ:

If the shock is a relaxer, what is the stresser?

LIDDELL:

The shock in one case was the stresser, in the other case the relaxer. The most amazing experiment which I accidentally did with this animal was the following: in preparing for the day's tests, with my elbow I accidentally closed a switch. The telegraph sounder emitted two clicks and I turned it off thinking the goat had not noticed. Seventy seconds later, he began lifting up the leg. Meanwhile, the heart-rate was climbing. And, at the start of the hundredth second, tense expectancy and tremendous stress was rapidly set up. So I say that we cannot use any rigid mechanistic thinking about the experimenter's control of the stressful stimuli. It is the meaning of the given stimulus applied to the animal which is of significance.

FOURTH DISCUSSION

Presentation: Dr. Whiting

FREMONT-SMITH:

We come now to Dr. Whiting's presentation which, like all the others, will be focussed exclusively on learning theory—and other topics.

WHITING:

And the psychobiological development of the child!

I want to introduce the notion of learning without direct tuition. It is what is suggested by the Freudian notion of identification and superego development; by the notions of imitation and the development of moral values. In every society and in every individual there are some internalized rules of conduct, which govern the individual's actions and made him feel guilty if he deviates from them. The strength of this internalization varies from individual to individual and from society to society. I do not quite know what word to use to describe this because I do not want to get into the quibble of saying that that is not what Freud meant by it, so I will use the word 'X'. I am going to propose that the more 'X' a person has the more he will resist temptation so that the first operational measure of 'X' is 'resistance to temptation'. The second thing this theory says is that if the person succumbs to temptation, either really or in fantasy, he will tend to punish himself. So the more 'X' a person has the more he will resist temptation to break the rules of the society in which he lives and the more he will punish himself if he fails to resist, either really or in fantasy.

Now as to ways which might cause differences in the value of 'X', there are the two intervening variables in Table 2, which I am going to call identification and evaluation. If you have a strong identification with a character who has strong values, then you get a lot of 'X'. Identification and evaluation cannot be measured; but I am

going to say that identification is primarily related to the dependency of the child, the need of the child for its mother. So parental behaviour causes differences in identification and differences in 'X' and is called the antecedent variable in Table 2. The first type of parental behaviour which relates positively to identification is nurturance — that is love, caretaking, etc. However, if the nurturance continues without asking the child to do anything in return, you do not get identification. The nurturance must be *contingent*, by which I mean

TABLE 2

<i>Antecedent Variables</i> (Parental Behaviour)	<i>Intervening Variables</i>	<i>Consequent Variables</i>
1. Contingent Parental Nurturance		
2. Technique of Discipline		
(a) Love-Oriented	Identification	
(b) Non-Love-Oriented		
3. Prestige of Parent		
4. Age of Onset of Socialization		
5. Strength of Parental Demands		
	Evaluation	
		(a) Resistance to Temptation
		(b) Self Punishment

that when the child begins to become a little man or a little woman the mother says, 'Well, now you've got to play your part of the bargain; you've got to be a good boy for me to love you'. If nurturance never becomes contingent you never get 'X' either by identification or evaluation. If, on the other hand, you never get any nurturance to start with you get a lack of conscience and probably a lot of other effects which are suggested by the unloved infant.

When socialization begins the child must be taught to be an adult who has responsibilities and operates under the rules of his society. In the first year of life he is permitted to do very much as he wants; he is nursed when he is hungry; he is permitted to defaecate and urinate freely; he is cared for with respect to cold, heat and so on by the parent. But this cannot always continue and socialization may be accomplished in a number of ways. It may be accomplished entirely by making love contingent on proper behaviour; but other techniques may be used, such as ridicule, physical punishment and so on. Now I want to separate the techniques into love-oriented and non-love-oriented and then I am going to say that love-oriented techniques are those which keep the child interacting with the parent or the mother.

MEAD:

Are you saying 'mother' or 'parent'? Do you mean 'mother' and/or 'father'?

WHITING:

I mean 'parent'. I mean anybody who plays the role of caretaking in early life.

MEAD:

Including the child's nurse and grandmother?

WHITING:

Theoretically.

As to non-love-oriented techniques, if you clout the child and it runs away and escapes, the contingency of nurturance has no longer any effect; and, rather than have a conscience, he tends to get away with whatever he can.

There is another thing about the parent that has to do particularly with identification. By identification we mean wanting to be or behave like the parent, so that the greater the power and prestige of the parent, the more the child will be likely to identify himself with him. (Table 2, No. 3.) Let us assume that both husband and wife share the nurturance and disciplining equally; but one is a henpecked person or a person of low prestige within the family set-up, and the other one is the admired person. The theory says that the child will identify with the more admired and more respected of the two, whichever this is.

GREY WALTER:

Is there not a risk of a tautology there? The more likeable person will be more admired.

WHITING:

No; the more a father is esteemed by his wife, the more the child will also esteem him and identify with him.

MEAD:

Are you also including social esteem? The father is the most important person, regardless of personality.

WHITING:

Stated cross-culturally this theory would say that if women in a society had a very low status but did all the nurturing and disciplining, you would have less of 'X' in this society than in a society where

the same was true about nurturance but the mother had a relatively high status.

GREY WALTER:

If you have a chief or a big shot of some kind who in his home is henpecked because of the personality of the mother, what does your theory say about that?

WHITING:

It would be a matter of empirical determination.

BINDRA:

Would you be willing to put as sub-headings under 'Prestige'—'Extrafamilial' and 'Familial'?

WHITING:

All right. To continue (Table 2, No. 4): I am going to say that identification depends not only on the amount of nurturance but also on the age of socialization: the process of identification and evaluation may be accentuated if it occurs at the time of the crucial age period. This is another hypothesis that this theory is going to ask questions about.

Finally (Table 2, No. 5), something which relates to evaluation is the strength of discipline—that is, how strong the values are that the parents hold. You may have a parent who feels, 'Well, you can do anything you want; I do not really hold anything very important about morals', and in that case you might have high identification, but you would not have very high 'X'.

ZAZZO:

I wonder if the diagram you have drawn takes into account the reciprocal aspect of identification? One generally thinks of identification of the child with the parent. Consideration should certainly also be given to identification of the parent with the child, as regards both sexual polarization and all other types of behaviour.

WHITING:

The point is very well taken, I have not got it in and I probably should have. It probably varies with the amount of nurturance that

the mother gives to the child. I would have said that the more the mother loves the child the more she would be likely to identify with it.

MEAD:

You are talking about boys, by and large, are you not? The chief point where reciprocal identification comes in is when the parent says 'This child is my sex' or 'This child is the opposite sex'. That is an important element omitted here, but as theories like this are never about girls it does not matter much!

WHITING:

The identification is also close with the parent of the same sex.

Our next job is to translate self-punishment into a particular measure, and the particular measure I am going first to use I shall call patient-responsibility.

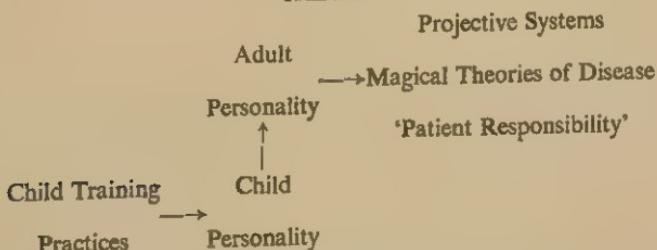
FREMONT-SMITH:

As distinguished from impatient responsibility?

WHITING:

No! Patient in the medical sense. In seventy-five different societies we had measurements of the child-training practices including all of those items mentioned in Table 2.

TABLE 3



Our assumption now was that if these child-training practices had an effect on the child which persisted to adulthood, these personality traits in adulthood would be reflected in the projective or magical belief system of the culture (Table 3). (I have slipped in another assumption here, that the child-training practices in the belief systems change at relatively the same rate, or they do not change at all.)

Patient-responsibility is part of this magical belief system. In most primitive societies the cause of sickness is attributed to some agent. This agent may be the patient himself or some other living person, or a ghost, a god or a spirit. A high patient-responsibility culture is one in which the patient himself is responsible for being sick because, as he says, 'I must have broken the festival taboo', or some such.

FREMONT-SMITH:

Self-responsibility for illness.

WHITING:

That is exactly right.

MEAD:

That is one end of a continuum.

WHITING:

The other is that I am sick because of the whimsical action of some sorcerer, ghost or spirit which brought the sickness on through no fault of my own. This is low patient-responsibility, or no patient-responsibility—where one does not accept this self-blame.

TANNER:

High patient-responsibility is the idea of sin then, is it not?

WHITING:

Yes; you are sick because you have sinned. There are societies where all sickness is thought to be due to sin, and there is none where the patient is believed to have no responsibility, but there are many where the responsibility of the patient is negligible. All societies can be put on the scale in the degree to which the patient accepts responsibility, and I am taking this as a measure of the strength of 'X'. High patient-responsibility, high 'X'; low patient-responsibility, low 'X'.

MEAD:

You are not including in the high patient-responsibility the people who feel, 'I am sorcerized because I am successful', are you?

WHITING:

I am.

MEAD:

In addition to sin; success, achievement, beauty, love and wealth?
If I am sorcerized, it doesn't matter what for?

WHITING:

If I accept this diagnosis, no.

BOWLBY:

Supposing *A* thinks that someone else is sorcerizing him because of something he (*A*) did, that involves the sorcerer as well as *A*.

WHITING:

Yes. But if *A*'s action completely determines the action of the sorcerer, then the culture gets a fairly high patient-responsibility.

BOWLBY:

It is a matter of degree as to what extent the sorcerer is a free agent and to what extent influenced by *A*?

WHITING:

Yes. The highest degree would be when it was believed, 'When breaking a particular taboo, automatically the sickness came upon me, completely, inevitably, without any question'. If there was some mediation of a sorcerer who had some free will of his own, then this would get a lower score.

BOWLBY:

And if it is pure whimsy?

WHITING:

If it is pure whimsy it gets a still lower score.

Now I am going to see if there is any relationship between the score for patient-responsibility and the type of child-rearing; our theory says they should be related. I am going to take age as the first variable to test. The theory states that if the dependency relationship of the child to the parents is maximal at a critical period then 'X' and hence patient-responsibility have got to be related to that critical age. The data are presented in Table 4; the mean scores on our scale of patient-responsibility are given for the different cultures who socialize children in the various listed ways at different ages.

Let us take the age of weaning first (Table 4, second column), the highest scores for patient-responsibility are in societies with early weaning.

BOWLBY:

What is the range of the scale?

TABLE 4. *Relation between Patient-Responsibility for Illness and Estimated Age at Onset of Various Aspects of Socialization*

The table shows the mean index of patient-responsibility for societies with various estimated ages at onset of each aspect of socialization; in parentheses after each mean is shown the number of societies on which it is based. The age intervals are not of uniform size, having been selected to avoid excessive bunching or spreading of cases in any of the five distributions. In the last line of the table are correlation coefficients expressing the closeness of relation between the index of patient-responsibility and estimated age at onset of each aspect of socialization. Coefficients marked with an asterisk are significant at the 5 per cent. point; those marked with two asterisks are significant at the 1 per cent. point.

AGE AT ONSET	ASPECT OF SOCIALIZATION				
	Weaning	Toilet training	Modesty training	Training in heterosexual inhibition	Independence training
Below 1.0	11.0(2)	4.0(2)			
1.0 to 1.9	11.6(5)	11.9(8)			
2.0 to 2.4	11.0(10)	11.7(7)		15.8(4)	
2.5 to 2.9	9.1(9)	9.0(1)			9.9(8)
3.0 to 3.9	9.5(4)	14.0(1)	13.8(4)		9.0(9)
4.0 to 5.9	4.0(2)	8.0(1)	10.5(3)		9.2(11)
6.0 to 7.9			9.2(5)	12.1(7)	6.0(1)
8.0 to 9.9				9.0(3)	
10.0 and above			9.0(1)	5.5(4)	
Correlation coefficient	-0.42**	+0.21	-0.50*	-0.74**	-0.34*

WHITING:

From 0 to 21; the mean is between 9 and 10. Let us not take this too seriously, but let us just say—since the correlation coefficients are most significant—that there is a suggestion that the age may be a factor of importance, not only in the immediate behaviour of the child, but also in the permanent development of the 'X' factor if we accept patient-responsibility as a way of measuring this.

BINDRA :

The joker in all this is the relation that you postulate between patient-responsibility and the strength of the superego. Would you like to say anything about why you think there is this relation?

WHITING :

It is a good question. Obviously I am not particularly interested in magical theories of disease in primitive societies. The theory came before the choice of patient-responsibility. I said, 'Now, what kind of a measure can I get out of this kind of material that might indicate self-punishment?'

BINDRA :

Your postulated relation refers only to the self-punishment aspect of the strength of superego?

WHITING :

That is right. We do not want to rest our case just on this one measure, but it might suggest that it is worth while looking for other, more satisfying and more direct measures.

MEAD :

You have made no attempt here to use a distinction that Erikson (1950) makes between sin as a product of certain situations in the first two years, and guilt in the four-year to six-year period, have you? Have you not got here a possible relation between autonomy around the age of two and patient-responsibility?

WHITING :

It could very well be so.

MEAD :

What you have defined are the cultures in which one says, 'I did it myself'. The emphasis is upon making the child do it itself early.

WHITING :

Maybe I could suggest one other thing. Another measure of self-punishment is the very common therapeutic practice of phlebotomy or bleeding oneself—cutting one's head, or wherever you have a sore spot—to get the bad blood out. This is in contrast to the other

therapeutic techniques where a salve or some pleasant thing is used for therapy. One might make the assumption that blood-letting was another measure of 'X' by the self-punishment route.

I am now working on this analysis, and it shows no very consistent relation to the age factor. It does, however, relate very significantly (1 per cent.) to independence-training.

I am working on a third measure and that is sacrifice to the gods as a therapeutic practice. This is also positively related to patient-responsibility, but there is no consistent relationship to the age factor. In superego development there is nothing except Table 4 which shows anything consistent about age. Sacrifice relates to training in control of aggression, however.

FREMONT-SMITH:

What do you mean by sacrifice?

WHITING:

Giving goods to the gods.

GREY WALTER:

Do you include paying fees to the doctor?

WHITING:

It does not include that; it includes giving something to the gods, some specially costly gift.

LIDDELL:

In this self-punishment concept, is there a special case of a suicide or all-or-nothing phenomenon?

WHITING:

Yes. But the data here are so inaccurate that I could not use them as a variable.

FREMONT-SMITH:

Those types of culture where individuals believe that they must lie down and die because they broke a taboo, would they be the ultimate in patient-responsibility?

WHITING:

That is right.

MEAD:

Then you put the Maori at the top?

WHITING:

The Maoris are at the top; they have the highest score of patient-responsibility. The Manus have a score of 14, the Arapesh of 11 and the Samoans of 9.

BINDRA:

Dr. Mead, does that fit in with the general impression you have of these groups?

MEAD:

I think there are two mixtures, as I have said. If you include dying for sin and dying for success, if you then add in autonomy with the emphasis on the period from two to three, this is what happens. But it is questionable whether to regard dying because you are successful, or because you are beautiful in a non-puritanical society, fits into the scheme of superego development. This sort of scheme is perfectly accurate, but may be an artefact of the societies of which we happen to have descriptions. Imagine that instead of a hundred societies we have descriptions, say, of thirty. You can get certain artefacts that have real regularities in them but are only part of the whole. Would you agree with that statement, Dr. Whiting?

WHITING:

Yes, I agree absolutely. This whole thing is an attempt to get out some of the variables in a problem that is exceedingly complex. Let us say that something looms from it, but not come to any conclusion.

TANNER:

May I get this clear in my mind—you are arguing from what we should call between-culture statistical relationships to within-culture statistical relationships, are you not?

WHITING:

That is right. What I am saying essentially is that an individual habit is the equivalent of a custom.

TANNER:

I am saying the same thing in statistical language.

WHITING:

The unit of culture represents the behaviour of a typical individual in a society. If you like, the individual is equivalent to society. So the customs of society are like the habits of an individual.

TANNER:

That is what is worrying me about your approach, the going from the between-society to the within-society.

WHITING:

It seems to me it is legitimate to say there may be something that can be discovered at one level that cannot be discovered at the other, and vice versa.

BINDRA:

You probably have to make different assumptions in order to be able to test the hypothesis at the individual level.

TANNER:

It is rather analogous to arguing from the pathological case back to the range of chemical variation within normal persons. Sometimes you hit it off and it saves a lot of time, other times it is, in fact, a *non sequitur* and breaks down. Every time you suspect something from a pathological case you have got to go back to the normal group and make sure that a similar thing does happen in a normal group; it may not.

WHITING:

That is exactly the design of our research.

The next step is a series of individual studies within not only our culture but several others, to check the hypothesis developed cross-culturally. My ultimate aim would be a theory which could account for relationships between customs in a society, and habits in an individual. No doubt they will not be just the same, but I would like to have the whole theory embracing both.

Almost all of our developmental theory is derived from individuals within a single Western European culture, and so one of the things that we really must do is to try out these principles elsewhere, because they may be cultural artefacts.

Contrariwise, we do not want to stick to this cross-cultural approach and say, 'Well now, this is going to be reapplicable back to our own culture without any corrections'.

Let me go back now to the hypothesis of Table 2. We were able to get a score for each society of the relative importance of love-oriented techniques, and found this was related to patient-responsibility at the 5 per cent. level of confidence. This is one of the few antecedent variables that has been consistent in its relationship to whatever measure we take of 'X'. The strength and severity of discipline in weaning, toilet-training, sex-training, independence-training and training for the control of aggression have the following correlations with patient-responsibility: weaning, +0.25; toilet-training, +0.06; sex-training, +0.02; independence-training, +0.18; aggression, +0.28. It is interesting that though they are all positive, the only one that is significant at the 5 per cent. level is aggression.

FREMONT-SMITH:

One might anticipate that training for handling aggression would be associated with self-punishment.

BINDRA:

Are these five variables themselves not related?

WHITING:

Very little; if society is severe in weaning, it is not necessarily severe in toilet-training. The full data are given in the book (Whiting and Child, 1953, p. 116). I ought to say that these ratings are judgements made by three judges who had a defined set of scales to use. The reliability of judgement was about 0.75 to 0.80.

BINDRA:

Did your three judges know your theory?

WHITING:

No, they did not, nor did they have any access to the data on magical theories of disease for the most part, because most of this was done on the data from the cross-cultural survey, where the information on child-training is separate from the information on theories of disease.

MEAD:

They were presented with paragraph descriptions, were they not?

WHITING:

That is right.

MEAD:

And these were lifted out of the ethnographic descriptions?

WHITING:

Yes.

Let me go on now to the next method of testing this theory. A group of my research assistants went down to the American South West to three societies and made individual-difference studies within three cultures. Two of the cultures were western European—one Mormon, the other Texan—and the third was American Indian, the Zuni. They got scores on approximately thirty mothers and thirty children in each culture and tested the hypotheses on individual differences in three cultural contexts. In addition to this, we are doing the same thing on a large group of four hundred mothers in a suburb of Boston. Except for a preliminary analysis that has been done by Chris Heinicke (1953) on a small part of the sample, these data are still in embryo.

In order to get an individual test of self-punishment we decided on a picture test, of a Thematic Apperception kind, in which we used dogs for actors in order to get rid of cultural artefacts—or attempt to. The dogs were all doing naughty things, like breaking things apart, fighting, stealing a bone from another dog and so on. These were presented to the ten- and eleven-year-old children just like a T.A.T.: what are the dogs doing, what are they thinking of, what will they do next? This was then scored for self-punishing responses, that is, self-punishment with respect to the dog. The respondent says that the dog feels sorry, or he ran and fell down and hurt himself, and so on. We were then able to get quite a surprising number of self-punishing scores. It is what you might call a projective self-punishment test.

In the Mormon and Texan groups, there was a positive relationship between the amount of initial nurturance and the amount of self-punishment. This was not true of the Zuni, partly because we were unable to get a stable amount of initial nurturance.

BOWLBY:

What age were the children?

WHITING:

They were in the sixth, seventh and eighth grades; the test was administered in the school, and it was a pencil-and-paper test.

TANNER:

How did you get the rating of the nurturance of the same children?

WHITING:

It was done by interviewing the mothers of these children.

MEAD:

And it was retrospective in regard to the children?

BOWLBY:

It was what the mothers had claimed they had done?

WHITING:

Yes, so we get pretty tentative scores on both sides.

With regard to sex differentiations we got two interesting results. When talking with the parents we asked them which one was responsible for policy with respect to child-training; when there was any decision to make about bringing up the child did the father take it, the mother take it, or did they share it? Thus we got a responsibility-for-policy score, and we found that where the same-sex parent was the one who took responsibility for policy, you had high 'X'.

Secondly, on a cross-cultural basis, with respect to the cultural prestige of men and women, the Mormon males are very high and the Texan males are low, with respect to the household, where the woman is the boss. The highest projective self-punishment score was found in the Mormon boys, and the next highest in the Texan girls.

Heinicke has made a study of five-year-old children using an individually administered test with dolls to give a self-punishment score, and found that the greater the nurturance the more the self-punishment, the more the denial of love as a technique the more the self-punishment and, finally, the more severe the punishment for aggression, the more the self-punishment.

LORENZ:

I should have thought that the positive correlation between the taboo and punishment of sexual activities and self-punishment would be still greater than that between aggression and self-punishment.

WHITING:

That was what I expected, because of the Freudian Oedipus resolution. The severity of punishment for sex in each one of the

studies has been tested with each of the measures, but in no instance has it related.

ZAZZO:

We have recently carried out a fairly extensive enquiry in a number of countries regarding the way young children imagine their family. This enquiry was rather rapidly carried out and the main test consisted of the well-known test of drawing the family.

We have not yet finished working out the results but we have made an unexpected discovery. We have tried to assess the valuation of the mother and of the father by boys and girls. In all the social classes and in all the countries where the enquiry was carried out the mother was more highly valued than the father. There is a fairly significant difference: in Italy, for example, the mother was more highly valued in the drawing, if one takes into account certain aspects, especially the height of the mother, which is greater than that of the father, and the execution and finish of the drawing.

That is the first thing, which is perhaps not so surprising in itself. But if we evaluate the drawings according to other factors, especially the frequency of representation of the father or the mother, or the order of drawing the father and the mother, we note that it is not the mother that is preferred by the same children, but the father.

Thus two evaluations are obtained, and this holds good for all countries. We have examined approximately three hundred children in each country.

FREMONT-SMITH:

What age were they?

ZAZZO:

Four, five and six-and-a-half, the pre-school age. We carried out the enquiry in kindergartens and nursery schools.

The question I want to put here is the following: as regards what the child takes as a model, are there not very different levels of evaluation, one being purely affective, which holds good equally for boys and girls, and another of a social order, which favours the father rather than the mother?

The figures we now have show this distinction very clearly with no appreciable differences between the two social groups we have observed.

As regards valuation on the social plane, we have wondered whether verbal expressions might not play some part! While the

child is drawing there is a verbal enumeration—for the French it is 'Father, mother and children'—which would result automatically in the father being drawn before the mother. The boys tend rather towards the father and the girls rather towards the mother at that age. The essential point I want to stress is the double aspect of valuation as regards the parental model—from the affective point of view and from the social point of view.

WHITING:

I would like to see the result of that. We made another study on the effect of the father, differentially from the mother, on the projective test of self-punishment. In this study, what the mother did was the only thing that had any influence on self-punishment, with one exception. Some of the fathers did a lot of the nurturing of children and some did not; this made no difference. Some fathers were severe with the children and some were not; this made no difference. The one thing the father did which did make a difference was the degree to which he esteemed the mother. If the father said the mother is doing a fine job in child-rearing, this child had a high 'X' or superego. If the father said he did not think she was doing such a good job, they had a low 'X'.

Another interesting thing here is that this was positively related with the mother's sexual anxiety. The mothers that had high sexual anxiety were not esteemed by their husbands, and their children had a low superego. The role of the father in this particular study seemed to be in support of the mother and not directly influential on the child. This was with five-year-olds. Whether they would change in later life I do not know.

MEAD:

How about the reverse position, the mother's estimation of the father? Did that have no effect?

WHITING:

It was hard to say. Generally speaking, when the husband esteemed the wife the wife esteemed the husband, so that it was hard to determine, but in so far as we could pick the two apart it seemed that the father's estimate of the mother was more important.

MEAD:

These were American children?

WHITING:

Yes.

GREY WALTER:

Was it explicit or implicit esteem?

WHITING:

During the course of the interview, we asked the father to describe how the mother was doing with her child-rearing; we probed on this, and from this were able to get a judgment of whether he thought she was doing a good job or not.

GREY WALTER:

Would the child have been aware of the father's attitude to the mother as an explicit statement such as, 'Your mother is a wonderful person' or is it simply that the father is leaving the job to the mother without comment and therefore implicitly approves?

WHITING:

The theory that I am working on suggests that the child must in some way know, either directly or indirectly. It may be just the mother's self-esteem and self-confidence that makes her a better model for the child to identify with.

Another thing is to do with concentration of authority on the two parents. The parents are the nurturers, but in certain societies the disciplining is done by a so-called disciplinarian—a typical case happens in the south-west—called kachina. Here the mother takes care of the child regularly, but somebody dressed up as a ghost, god or spirit comes and visits them. He knocks on the door and says, 'Have you got any bad little boys there?' and the parent says, 'Well, Jimmy hasn't been very good but please don't take him away'. The spirit will then say, 'We have got to take him in our basket' and the mother will say, 'I am sure he will be good'; he will then say, 'Well, are you sure we shouldn't take him?' and finally the mother pushes the kachina out of the house, but the kid is plenty scared and has got plenty of discipline to last him for several weeks. What does this produce, low or high superego? Our theory would say low, because you have got the separation of nurturance and anxiety, the mother standing constantly for nurturance.

The kachina is characteristic of the Zuni and the Zuni have in fact one of the lowest patient-responsibility societies in our sample. An

interesting confirmation of this is the sample of individual Zuni children within Zuni culture. There were some parents who used denial of love rather than calling on the kachina to visit them, and in these families we found relatively high guilt. With those who called in the kachina we found a relatively low guilt.

LORENZ:

How often does the kachina come?

MEAD:

Irregularly, mainly in connexion with the big dance feasts. It is very much like the Black Peter, Caspar and Santa Claus—that sort of thing. You build up to the fact all the time that the kachina may come, and then the parents tell the kachina how much punishment they want meted out, and then play the role of being the children's defendants. You have a high degree of insincerity in this picture.

BOWLBY:

Do the children see through that?

WHITING:

I think they do to a certain extent, but there is always a haunting doubt.

There is one other fact of social structure that we can bring in here, and that is polygamy versus monogamy. This comes out as expected. In the monogamous families patient-responsibility is high and in the polygamous it is low. This is over thirty-four societies.

GREY WALTER:

Have you any record of any polyandrous society?

WHITING:

Only one, and it had an about average patient-responsibility.

MEAD:

Have you tried taking ten societies where you had a very large amount of good, comparable data and where you would expect to get significant results? I should think that today the available mathematical methods of studying small groups would make it pay to work with a smaller number of good data.

WHITING:

That is true. But there is always a point of diminishing returns. If the number is too small, you cannot do anything. What we want is more cases better covered. If we have a lot of cases which are good, then we can hope to find something.

MEAD:

In the study of Ford and Beach (1951) which was done on the same sort of material, there was a claim of something like one hundred and nine societies and in forty per cent. the basic incest details were not given correctly. It is dangerous to use material on which you have not good data.

TANNER:

How many societies do you reckon there are good data on, Dr. Mead?

MEAD:

If you want exceedingly complicated points, probably not more than a dozen. I would not attempt to do what Dr. Whiting is doing with more than twenty.

WHITING:

There are about fifteen to twenty of these seventy-five societies in which you have a fairly good number of reliable statements. There are a lot more, however, from which you can get a pretty specific statement about one particular aspect of child-training, such as a very concrete and explicit statement about weaning and nothing else. Whenever I got something like that I included it in the sample, so that I could use it for weaning even if nothing else.

MEAD:

I do not believe that if you do not look at anything else you get the weaning right.

WHITING:

That may be so.

BOWLBY:

I should like to raise the whole question of the relationship between patient-responsibility and what we mean by superego. It

seems to me that so far what we have been discussing is the relationship between patient-responsibility and all these different variables. How patient-responsibility relates to the superego does not seem to me to be self-evident. That it is related I do not doubt, but it does not seem to me to be necessarily very closely related. The superego is rather complicated and contains more than one variable.

GREY WALTER:

Can you measure the superego?

BOWLBY:

You cannot.

GREY WALTER:

Then what is the point of discussing it?

BOWLBY:

I think it is useful to try and see how things relate to these psychological functions after which we are dimly trying to grope. The question in my mind was this: how, in a culture, does patient-responsibility relate to law-abidingness: does patient-responsibility relate to successful resistance to temptation?

WHITING:

I have no evidence on this. I would say that even a very law-abiding person, unless he has a very rigid superego, may still be tempted in fantasy.

BOWLBY:

Do we really object to people being tempted in fantasy?

WHITING:

We do not mind about it, but I think you are still going to get evidence of guilt, whether we mind or not. It seems to me that even though we only punish the actual occurrence of deviation, the individual himself who has a strong value for 'X' punishes himself for the fantasy.

MEAD:

There is an aspect that we have not touched on yet—the question of external and internal sanctions. The Manus believed that people were made ill because of their own sins. They had a moral system

which was so arranged that you could never be good enough, because the next time you had an attack of malaria somebody would find something you could have done which you had not done yet, so it was impossible to be permanently good. You could always build a bigger house, feed another pig, make another trip, or something like that. Illness was a punishment that showed that you had done something that you ought not to have done, or had not done something that you should have done. So the Manus would get a very high rating in patient-responsibility. Twenty-five years ago, the sanction was externally imposed for acts of commission or omission. A diviner divined why you were sick, you promised to build the house or pay the debt, and you got well. Internal sanctions were relatively low.

Now the Manus have moved in twenty-five years to the position where the most serious thing is *thinking* about doing something wrong, which is far more serious than doing it. *Acts* are now in the province of civil authority and they do not cause illness. If you take your neighbour's wife, the court will deal with you and fine you or put you in jail, but if you are angry with your neighbour for taking your wife, you get sick and you may die. So fantasy and thought are the major sins, though the people would still rank high on law-abidingness and on resistance to temptation. Illness and death are still the results of your own behaviour, but whereas it used to be in action, now it is in fantasy.

WHITING:

There is another study in progress in which a patient-responsibility is being related to taboos, particularly food and sex taboos during pregnancy. There is a very high negative relationship between taboos and patient-responsibility, suggesting that taboos are an externalized form of control as contrasted with patient-responsibility. But in this particular study I do not know whether we would give a different score to the Manus now and then.

MEAD:

You included fantasy behaviour, did you not?

WHITING:

I did in my definition of self-punishment, but not in my definition of patient-responsibility. Law-abidingness can result from fear of external punishment or from superego.

BOWLBY:

Or from both.

MEAD:

Or it can be the Balinese position, where you have a preference for balance built into the personality, which is another level entirely (see Bateson, 1949).

WHITING:

By law-abidingness I mean something a little different from resistance to temptation. I mean resistance to temptation with the possibility of external punishment removed.

BOWLBY:

Surely that is a mythical case, is it not?

WHITING:

No, I do not think so. There are instances when one might be tempted, with not the slightest possibility of getting caught, but you do not do it if you have a conscience. 'Thus conscience doth make cowards of us all'. There is no society which depends completely on internalized or externalized control; it is always a mixture of the two.

Let me tell you about an experiment on resistance to temptation in dogs. We got eight puppies and brought them up just about the way Dr. Bindra did. For four months, four of the puppies, starting at six weeks old, lived in isolation and were fed by machines. The other four were not in houses but in the laboratory where they were fed and petted by humans. So we have got a machine-fed group and a human-fed group.

First we tested these in terms of the dependency measure that Scott (1951) used: you put your hand in the cage to see if they will lick it or not. Two of the machine-fed dogs and two of the human-fed dogs licked. This was clearly the result of species temperamental differences and not the result of training.

We were going to try and give the dogs a superego and were going to ask the question: 'Does machine-feeding lead to higher superego or "X" factor or whatever it is in dogs?' I was betting against this. My colleague Dick Solomon said a dog certainly could be given a superego. Anyway, after this differential training, the experimenter sat down with horsemeat on one side and dog chow on the other and a newspaper in his hand. When the dog ate horsemeat, he would whack him over the nose, and when he ate dog biscuit it was O.K. After a relatively short time, all the dogs ate the dog biscuit and did not eat the horsemeat; so let us say we have made the evaluation—the parent has made the evaluation—that you must not eat horsemeat but you can eat dog chow.

Then the dogs were put in the temptation situation, in a room by themselves with the experimenter looking through a peephole. How long would it be before the dogs ate the horsemeat? Was there any difference between the machine-fed and the human-fed, in resistance to temptation by the horsemeat?

BOWLBY:

Who did the whacking?

WHITING:

The person who fed them.

TANNER:

How many whacks did each dog take?

WHITING:

The human-fed dogs took less whacks than the machine-fed dogs, they learned quicker.

TANNER:

Then it might go either way.

LORENZ:

I am quite sure that a really well-trained dog can be left in the situation of temptation for ever and he will not eat the horsemeat; but my prediction is that these puppies who had just been whacked two or three times had not developed a superego. They may have, but I do not believe it.

WHITING:

The machine-fed dogs resisted temptation from thirty seconds to six minutes—that is the range of the four dogs; and the human-fed resisted from six minutes to six hours. One of these human-fed dogs was four days hungry before he ate. He was brought in on four successive days, he ate the dog chow which was there, a rather small amount, and was left there for half an hour and was then taken out until the next day, when he was brought back and again ate the dog chow but did not eat the horsemeat. It took him four days before he finally broke down.

LORENZ:

May I tell two short stories about superegos in dogs? One of my dogs who had been reared with ducks and geese and other fowl, and who quite certainly had never killed one and been beaten for it, accidentally killed an old gander who bit him in the tail so severely that the dog gave a reflexive snap and the gander who was twenty-four years old and had somewhat weak bones got a slight fracture on the back of the skull. The dog got a neurosis from that to the extent of hiding in a garret where none of our dogs had ever been seen; he crept behind some boxes and stayed there, not coming out for more than twenty-four hours; we only found him on the second day.

The other story is this. One of my bitches showed very strong superego when in the situation of temptation with lambs bleating and chickens fluttering about in the farmyard; she would put herself on the lead, coming to heel spontaneously whilst she trembled with temptation. In order to prevent herself being tempted she created the fiction of being on the lead. I think that bitch would have starved to death rather than kill and eat a goose.

FREMONT-SMITH:

In these experiments you did not expect the superego to develop so quickly?

LORENZ:

I should have thought the very short influence of man would have been insufficient to develop any 'superego' at all.

BOWLBY:

It is possible that these puppies that had been hand-fed had been submitted to a good deal of training by the feeder over the preceding months.

GREY WALTER:

Would someone explain to me why the superego has got involved in these experiments?

TANNER:

I was just about to ask Dr. Grey Walter what the relation of superego was to conditioned reflex experiments!

MEAD:

You need to bring in here the relationship between learning in the parent/child situation and affection, so as to differentiate between the human-fed and the machine-fed puppies. Otherwise, you could administer conditioned reflex training and you would have no characterological concept, no intervening variable to explain the difference. The superego idea is probably too complex to use here, but you need some intervening notion.

GREY WALTER:

I do not see the necessity. As Dr. Whiting has described the experiment, the human-fed animals were human-trained animals and they had learned from humans.

WHITING:

Let me ask a sixty-four dollar question. We are going to run a control and have the machine educate the dogs. The machine feeds and punishes both groups. What is your prediction? Which group is going to resist temptation here?

GREY WALTER:

Are they going to be given their instruction in the test situation by a machine or by a human?

WHITING:

By a machine.

LORENZ:

The answer is how much the machine fits into different I.R.M.s waiting for a parent figure.

WHITING:

If it should come out that the human-fed animals would resist temptation even when the machine punished them, I do not know how I would interpret that. If the machine-fed animals came out resisting temptation when the machine punished them, I would say it was punishment by a nurturing stimulus. A stimulus held to be associated with positive valence has more strength than one that is neutral. Five punishments by the hand that fed you counts twenty-five, so you get more inhibitions. I am going to bet a nickel that the dog identifies with a human and not with a machine. That is the

essential difference. I think you are going to have to have some kind of hypothesis like identification, some kind of intervening variable.

BINDRA:

Before you do that, you have to use the control that Dr. Bowlby suggested. These human-fed animals are also wandering around in the laboratory, and they probably get some training which the machine-fed animals do not get.

WHITING:

No, they do not. They are all in cages, and they all have the same routine. The only difference is that once a day, for about ten minutes, with the human-fed dogs some person comes in and gives them the food. Otherwise they are treated in a completely standard manner.

BINDRA:

So they are not human-reared, they are just human-fed?

WHITING:

Yes.

FREMONT-SMITH:

You also have to test the limits of the machines. It is quite possible that the dog would not identify with one machine, but if you made an appropriate machine, a warm machine which had certain odours to it and a vocal magnetic tape inside it with a human voice and so on, you could get a machine which was quite within the limits of an I.R.M.

LORENZ:

Try to make a supercanine machine which fits the dog's I.R.M. better than a man does; which, for instance, snarls when punishing and wags a huge tail when pleased!

WHITING:

In other words, if this comes out in this way, the perception of similarity between self and punisher is going to be crucial.

TANNER:

This brings us to what I want to ask Dr. Lorenz. What is the ethological equivalent of identification?

LORENZ:

It is already complicated, because it means social attachment to a social superior. But to make clear the difference from a simple conditioned reflex: the point in my first story was that this dog had never been beaten, but behaved as if it anticipated a horrible beating.

GREY WALTER:

You mean he imagined something he had never seen?

LORENZ:

No, he had traumatized himself by doing something contrary to any formerly accepted custom of this society. I would mention the case of a dog biting a human by mistake, for instance a dog biting a human who tries to separate fighting dogs. A normal dog who has never bitten a man, who has never been punished for biting a man, gets a neurosis to the extent of trembling, not being able to walk, breathing irregularly and breaking down completely. That is for doing something for which he has never been punished, but which is contrary to some laws of society.

FREMONT-SMITH:

Contrary to his expectations of himself?

LORENZ:

Yes, that is quite true. Then I have another point. This bitch of whom I told you, who put herself on the lead of her own free will in order not to follow temptation, broke down completely when isolated from me. She did it twice, once when I became a Professor in Königsberg, and once when I was recruited into the army. She lost every type of behaviour which was socially learnt. She lost her house-training, and she killed everything she could get hold of. This is also interpretable in terms of neurosis, but you may also say that all these higher social performances of being house-trained and not killing chickens were dependent on the presence of an individual. It is only saying the same thing in different words.

FIFTH DISCUSSION

Presentation of Film by Dr. Bowlby

BOWLBY:

The purpose of this film* is to show how a child responds in a separation situation.

A great deal of work on separation and its effects has been done in terms of long-term separations and the long-term effects; we know them sometimes to be very damaging. More recently, however, we have been concerned with trying to understand what happens at the time of separation and during separation; this is the first cinematograph record we have made of this event. It was made in a hospital because we could foretell that this child would be going into hospital and would only remain there a short time. The child, who is quite healthy before the event, had a small umbilical hernia to be dealt with, a small operation which is known not to cause too much pain.

We selected this child 'at random'. The reason we did it this way was because my colleague, Robertson, and I have so frequently been accused of exaggerating the emotional disturbance of these relatively minor social happenings: eight days in hospital—nothing very much—no real importance! And so we said, 'All right, we'll make a film of the next child to come in who conforms to certain criteria'. One criterion was that he or she must be between the age of 18 months and 2½ years; that is to say, after the child has made a differentiated and focussed relationship to a mother figure, which we know develops only slowly during the first six or nine months. This child, therefore, is well beyond the age when a differentiated relationship has been formed, yet still at an age when the attachment is at a maximum intensity. Her age proved to be two years and five months.

The second criterion was that we wanted a child from a normal happy home. The child selected, in addition to having loving parents, was the eldest child; there was no other child born then, but the

* JAMES ROBERTSON: *A Two Year Old Goes to Hospital*. 16 mm. B & W. Sound 45 mins. (Tavistock Clinic).

mother was four to five months pregnant at the time the film was made.

It was made in an ordinary children's ward, with a minimum of disturbance. Robertson just had a hand camera. There was no special lighting; it was made in the month of August when the light is good. There was no disturbance in the ward routine at any time and what is seen is just what happens to any child. There are two exceptions to that statement: one is that we asked that at a certain point in the morning of each day a nurse should come and play with the child, because we wanted to have a record of how the child treated the nurse. The second thing is that, because the child was being filmed, she did get rather more attention than she would otherwise have got; for instance her parents were allowed to visit her on four days out of eight, which we understand would not have been permitted otherwise.

The film was shot in two ways. Certain events—coming in, visits, and so on—were filmed as and when they occurred. In addition, each morning there is a period of time-sampling. It was known before the event that between 11 and 12 o'clock the ward was quiet; most of the surgical and medical activities had been completed and lunch was not yet coming round. Therefore, in order to avoid the criticism that we were merely selecting what we wanted to show and that it was a distorted picture of the responses, Robertson filmed by the clock; at 11 o'clock precisely there is an eight-second shot, at 11.5 another eight seconds and so on through to 11.40. It was during this time-sampling period each morning that the nurse was asked to come and play with the child. She came at 11.15 and, during the six minutes when she is playing with the child, Robertson (1953) shot eight-second sequences at one-minute intervals.

Synopsis

Laura is two years and five months old, a first child and so far an only one. She is intelligent, mature, and for her age has unusual control over the expression of feeling. She rarely cries. She is about to go into hospital for eight days to have a minor operation for umbilical hernia.

Although her parents had tried to prepare her for going into hospital, when she meets the admitting nurse she is cheerful and friendly and clearly does not realize that her mother will leave her. Going through the ward she seems less confident, and when she is undressed to be bathed she screams for her Mummy. Within 10 minutes, however, her exceptional control over feeling asserts itself and she is apparently calm.

She is put in a cot and breaks down again when nurse takes her temperature—'Don't like it. I want my Mummy'. A few minutes later Mother comes to say goodbye, and leaves for her consolation a piece of blanket she has had since infancy and which she calls her 'baby'. Throughout her

stay this 'blanket baby' and her Teddy make a link with home and are clung to when she is sad or frightened.

When alone she appears calm, but if a kindly person stops to talk with her her feelings appear. Sister comes to greet the new patient and Laura's face crumples -'I want my Mummy'. This occurred throughout her stay; the camera shows that what may easily be taken for calmness is often a facade which contact with a friendly person breaks down.

When the surgeon comes she clutches her Teddy and blanket 'baby' for comfort, and despite his tact she is apprehensive and resistive. Occasionally during the day she asks quietly for her Mummy, but without insistence.

On the *second day* she looks strained and sad, and has difficulty in responding to the nurse who comes to play with her. Then her feelings appear and she cries for a short time for her Mummy. But though she cries little throughout her stay, she takes great interest in other children who cry—as if they cry for her who is too controlled to cry. A rectal anaesthetic is kindly administered, but the strange experience frightens her. Thirty minutes after recovery from the anaesthetic her parents visit. She is very distressed—'I want to go home'—tries to get to her mother but has to be restrained because of the stitches, and rolls about on her pillow crying. As her parents leave she is subdued and seems perplexed. She waves slightly in response to their cheerful going.

On the *third day* she is seen quietly clutching her Teddy and blanket 'baby', not crying or demanding attention and likely to seem 'settled' to busy ward staff. But when a nurse comes to play with her she is at first withdrawn, then in contact with the friendly person her feelings break through again and she cries bitterly for her Mummy. When the nurse leaves her control gradually reasserts itself. This cycle of withdrawal, breakdown, and resumed control is repeated shortly afterwards when the nurse again plays with her. In the afternoon her mother visits, but although Laura has been sitting up all morning and has wanted her mother she makes no attempt to get to her. Mother would like to take her on her lap but is afraid to do so. Ten minutes later a nurse sits her up, but it is 15 minutes before Laura thaws out towards her mother. Then she becomes increasingly animated and friendly, and is transformed by a radiant smile seen for the first time in three days. When Mother says she has to leave Laura is immediately anxious, and as Mother leaves she turns her head away. She does not cry, but shows her feelings clearly by the change in her expression and the restless movement of her hands. Although it is the middle of the afternoon, she asked to be tucked down with all her personal possessions tucked around her and forbids the nurse to remove the chair on which her mother had been sitting.

On the *fourth day* she plays wildly with the hospital doll. She is not visited.

On the *fifth day* her mother visits in the afternoon, and again there is a period of withdrawn behaviour before she warms up to her mother. She asks once to be taken on to her mother's lap, but Mother, restrained by what she believes to be hospital regulations, says 'I'm afraid you can't'. Laura does not ask again. When Mother has to go Laura is pained, cries a little then quickly recovers and sits with pursed lips.

On the *sixth day* a new child is admitted who cries a lot. Laura, very controlled herself, watches him with a tense face. (When she got up she went to him and said 'You're crying because you want your Mummy. Don't cry. She'll come tomorrow'.) She is not visited.

On the *seventh day* both parents visit and Laura is up for the occasion. Although she knows chairs are being set out for her parents she shows no excitement, and when her mother comes she makes no attempt to go to her. She remains subdued. When Daddy comes from the office 10 minutes later he gets a warmer welcome. Daddy leaves first and his going is apparently almost ignored. Just once she says quietly, 'I go with you', but does not insist. When her mother leaves, Laura seems to ignore her going.

On the *eighth morning* she is shaken by sobs. Her mother had told her the previous evening that she would be going home today. Laura had kept it to herself. Now her control has given way. When Mother comes Laura remains cautious, however, and not until her outdoor shoes are produced does she accept that she is going home. She insists on taking all her possessions home with her, even a tattered old book she refuses to leave behind. (When she dropped that book on the way out and a nurse picked it up, she screamed in temper and snatched it away—the fiercest feeling she showed during her whole stay.) On the way out she is seen walking apart from her mother.

Subsequent history

Certain events during the next 12 months are of interest.

(1) For two days after discharge Laura was unusually anxious and irritable. Her voice took on a higher pitch. She slept badly. She soiled herself several times. But after two days her parents felt she was her pre-separation self.

(2) Four months later her mother went to hospital to have a second baby. Laura went to stay with her grandmother and did not see Father or Mother for five weeks. When she was reunited with them she recognized her father immediately and was friendly with him, but she failed to recognize her mother and for two days treated her politely but as a stranger. She remembered the whereabouts of everything in the home, but for her mother alone there was amnesia. She never spoke of hospital, and if anyone else referred to it she made no response.

(3) When six months home she was apparently her normal self and now talked of the hospital. By accident she saw a sequence from the film and immediately became very agitated. She flushed and said angrily to her mother 'Where was you, Mummy? Where was you?'. Then she burst into loud crying and turned to her father for comfort. Her parents were astonished by the strength of feeling which had been revealed.

(4) When 12 months home and again apparently quite serene she was taken by her parents to an exhibition and left in the creche there. She appeared quite ready to stay, but when a photographer appeared she became hysterical and it was an hour before she could be consoled. Apparently the camera was associated with the separation experience in

her mind. These two incidents suggest that despite her apparent recovery there remained deep-seated anxiety which could be touched off by trivial happenings.

FREMONT-SMITH:

We are now prepared to launch into the discussion of Dr. Bowlby's film. I wonder, Dr. Bowlby, whether you would like to make some comments at this point to start us off.

BOWLBY:

I should like to relate what we are studying in these children with what Dr. Liddell was saying about stress. He was defining stress as the physiological reactions anticipatory to exertion, and the de-stressor as the fulfilment of an expectation, even though what is expected is disagreeable. It seems to me Dr. Liddell has emphasized the physiological side of stress. I think one can also define stress psychologically by saying that it is any situation which arouses a drive in the organism which is not immediately fulfilled; and, the greater the drive and the more delay in its fulfilment, the greater the stress.

GREY WALTER:

The delayed cancellation of a drive or tropism at the simplest level is an unpleasant situation for us, but it happens so often that to use the word stress seems to me to be rather extravagant. It seems to me, looking at the film, that the time when a child shows most stress is when the statistics of the situation are not acceptable to her. She had been separated from her mother, she had been told this was going to happen; she kept looking out of the window to see if her mother was on the bus; she was wondering whether nurse was friendly; and testing things around her. No one could give her a firm answer. They did not tell her it was going to hurt. It is the fundamental uncertainty of the situation which produces stress rather than the fact that her desire to see her mother was delayed.

BOWLBY:

You mean the uncertainty of when she will see her again?

GREY WALTER:

And whether people around her are as friendly as they seem. There is a nice pretty nurse at the beginning of the film, but then Laura sees her no more. The food comes in, but it is perhaps not

the sort she really wants. Someone comes along and hurts her tummy, and so on. They are all minor assaults. It is the uncertainty, the insecurity, based on the statistical insolubility of the situation.

BOWLBY:

I think there is a continuum from the situation where a drive is aroused and is immediately satisfied to a situation where a drive is aroused and completely frustrated. Though it is a continuum, there may be a break in the middle of it. I agree that the word stress should be kept for one end of the continuum.

LORENZ:

I was wondering when somebody would bring up the very difficult question of where the 'normal' ends and the pathological begins. It seems to begin with the question of stress. Stress is what produces neurosis; stress is what the organism cannot cope with.

GREY WALTER:

Stress is a physical term, is it not? It is the force applied, it is not the deformation resulting. The physicist distinguishes in English between stress and strain; stress is the pressure, and strain is the tendency of a thing to bend or break under pressure.

FREMONT-SMITH:

Then you are driven to the position that, since the environment constantly produces a deformation effect upon the organism, life itself must be defined as vigorously stressful throughout; but these particular stresses we have been talking about have been specialized accentuations with a particular element of uncertainty in them.

GREY WALTER:

The time-factor is particularly important, as Bowlby has emphasized. He is speaking really not so much of stress as of strain, as irreversible effects in the organism. The organism is coupled with a certain environment; it looks round for some information on which to base a statistical evaluation. It fails to find it, and permanent deformation is then present and persists, even in other environments, as strain.

The capacity not to be permanently deformed by a situation depends upon the capacity to anticipate the mere passage of time. If you are put in prison for a year, you can say to yourself, 'Oh well, a year will pass'—but a child of two cannot do this.

HARGREAVES:

That implies first the capacity for the appreciation of time, and secondly the capacity to believe in the continued existence of objects when they are not there.

MEAD:

And also a learned expectation that things will turn out all right. At a very early age you can learn in two ways—that the next thing that happens is always going to be bad, or that the next thing that happens is going to be good. This probably is closely connected with the belief that objects will reappear.

GREY WALTER:

There is a phase in development when the child has not got the capacity to say, 'Time will pass'. You can see that in a two-year-old—you can almost imagine that you can see Laura trying to meditate on the nature of the eight days that she has to spend in hospital.

BOWLBY:

Most children of this age are living in the here and now.

BINDRA:

I think what Grey Walter is saying is quite right, but I do not think this is a general definition of stress. There are many other 'stress situations' which do not show the particular features of time and expectation; for example, exposure to cold. One has to distinguish between a stressful condition, a situation which can be defined in physical terms, and the *effects* of stress. I think yesterday Dr. Liddell defined stress not in terms of the situation or experimental conditions, but in terms of the psychological effects of it.

GREY WALTER:

That is strain.

BINDRA:

Yes. But Bowlby also is doing what Liddell did: he is defining stress in terms of the effects the particular 'stress situation' has on the subject.

On the other hand, the position taken by Grey Walter, with which I agree, requires that stress be defined in terms of the features of the situation; they may be statistical features. Adopting such a definition,

one can meaningfully state that the same stress has different effects (strain) on different people.

MEAD :

Then you would define stress situation in terms of species, and then differentiate among individuals of that species ?

BINDRA :

Yes.

BOWLBY :

I think stress is always relative to the particular individual organism.

BINDRA :

It is the effect of stress, that is strain, that depends on the individual, not stress itself.

BOWLBY :

I think, subjectively, we say we experience stress when we are very uncertain whether we are going to succeed. It has to be a situation which we take seriously and not one we do not care about. Our motivation is strong and we are faced with a problem which is on the verge of insolubility. It may be that we keep our heads and that we solve it ultimately. Alternatively, we may lose our heads and adopt crazy solutions. That may involve something quite different, but subjectively speaking we should describe both as stressful situations. (Schaffer, 1954.)

INTERVAL

LIDDELL :

At what age do you think a mother becomes a bad mother as against a good mother, being the same person?

BOWLBY :

Any time after nine months; any time after the mother is clearly defined as a figure, though I must admit there is at present a lot of debate in psychoanalytic circles regarding this age. Probably the maturation rate differs considerably in different children for this function as it does for others, and detailed research is badly required.

LIDDELL:

This child was old enough then to count cases of 'bad Mummy' and 'good Mummy'.

HARGREAVES:

This behaviour on the part of the mother begins to cast doubts on previous assumptions. I think you should emphasize what happened later.

BOWLBY:

Laura showed very little upset on returning home, though during the first 48 hours she did not sleep well and she clung to her mother more than usual.* This is a brief response compared to what is common, partly perhaps because this particular child is much more mature than others of her age; she can cope with time and space a great deal better than an average child of two years five months.

ZAZZO:

The film was most interesting and is directly related to our subject—as was shown by the discussion we have just been having—but when it is a question of proving to sceptics that the child who is separated from the mother shows very serious reactions, I do not think it is very convincing.

I am wondering about two other questions. To what degree are the reactions that we noted yesterday in a child of two and a half—who was very advanced and could have had a mental level of three or four years—comparable to the reactions which you yourself recorded on much younger children, of an age when the child is not yet capable of distinguishing his mother clearly. The reaction of the layman might be to say that Laura gets bored and that's all. There's nothing else necessarily in the film.

The second question is in connection with your previous work on hospitalism in very young children. A very large number of paediatricians would be sceptical about the generalization of the idea of hospitalism itself. You yourself, Spitz and others have always tended to generalize from the cases observed, and it is in fact possible that this is a general phenomenon which could even be found among animals too. This would be another investigation of considerable interest. However, you cannot convince the sceptics if you do not

* During these 48 hours her parents noticed that she was always near to tears, contrasting with her normal self, and that whenever either parent left the room she was upset. She demanded her mother sleep with her and called out in her sleep 'Don't do that to me'.

manage to show the frequency of deterioration due to hospitalism. Is it found in all children? It is this, and not the seriousness of certain reactions in certain children, which is open to doubt.

It seems to me that it would be fairly easy to carry out a statistical study on a hundred or two hundred children who had been separated from the mother, in order to discover the percentage of children who do not react in a pathological manner. I do not know if such cases exist, but if they do they would perhaps be exceptions proving the rule.

I have no doubt of the value of your work—moreover you will remember under what circumstances I met you: it was as an expert at a time when I myself was working on these problems of hospitalism; therefore I believe in them. But I wonder what actually is the extent of the phenomenon and I am sorry that as regards this question, which I also put to Spitz, no reply has been able to be given which entirely satisfied sceptics and myself—and I am not a sceptic.

RÉMOND :

I should like to ask what you think about the possible advantages of the stress which the film deals with?

Do you think that such a stress is always dangerous and bad and consequently that it should be systematically avoided or, on the contrary, that it is a factor which can be useful? In point of fact we develop within an environment which is a system of varied stresses, which create equally varied reactions, sometimes very strong. Are not the variations, the modifications, or even the intensity of these stresses very useful to our development sometimes?

Dr. Liddell has already given us his ideas on the question of stress among animals. But I should like to know whether in your personal experience, Dr. Bowlby, and particularly in the film you showed us, you do not think that in the occurrence of this stress there is a factor which may be beneficial?

INHELDER :

The question I am wondering about is whether there exist not merely one but several periods at which the child is particularly sensitive to separation.

Thanks to the work of Anna Freud, John Bowlby and René Spitz, we know the effects of separation towards the end of the first year, which is a critical period during which the concepts both of the affective object and the intellectual object are developed. But do we know the effect of separation at later ages? At about three years,

when the representations of self as opposed to non-self are formed, does the child react differently to separation? Finally, is the period of six to seven years, which seems to be a period of profound transformation of intelligence, a particularly vulnerable period from the affective point of view too?

Intellectual development, although continuous, does not keep to any constant rhythm. Periods of sudden transformation are followed by periods of slower adaptation. One might wonder whether the child at the turning point of its intellectual development shows an increased sensitivity to events producing trauma, such as separation from the mother.

BOWLBY:

Well, we had not realized that a child of this age suffers so much! How we evaluate the suffering may be controversial but it is clear that this child suffers in the ordinary human sense of the word. The film was not published to convince sceptics—one case could never do that—but to draw attention to a problem.

I recall one elderly nurse in a responsible position in this country who said to me very candidly, 'This film brings back to me the first child I ever nursed in hospital. This child was a little boy. He grieved for his mother and it simply broke my heart. After that I never saw grief again until I saw this film'. I think there is no doubt that she *could* not see it again. As a method of opening people's eyes to what in a sense they have always known—but to which most people have had to shut their eyes—it has been very fruitful.

We certainly cannot generalize from this film, and we cannot relate what we see in a child of this age to what occurs in a child of, say, under nine months—before the parent has been identified. It may be that they are two quite different conditions.

Dr. Zazzo raised the point that there are many children who escape severe or even any permanent damage, and he asked about the incidence of adverse outcomes. Our evidence is rather scanty. There is no doubt that many children go through what we should regard as severe separation experiences and come through them fairly all right. If anyone asks us what is the percentage of those who come through all right and those who are damaged, we do not know. But I do not think we need be disturbed by not knowing. I am now talking about the practical public-health issue. We know, for instance, that any infectious illness, a disease like poliomyelitis, say, only causes adverse effects in a minority of people. But the whole of public health has been built up on the simple basis that there are certain conditions which cause a sufficiently serious outcome in a

sufficient proportion of the population that we think they should be avoided. I think we can say, in regard to hospitalization and separation, that a sufficient proportion of children are sufficiently damaged for these conditions to be regarded as hazards to be avoided—at any rate to be avoided in their more severe forms and to be treated very respectfully in their minor forms.

Dr. Rémond raises the question whether these stresses can perhaps be advantageous to adaptation. We do not know. There must be a threshold below which it might be advantageous, and above which it is disadvantageous; and this threshold, of course, will vary greatly with the age of the child. My own suspicion is that anything which makes an individual lose his head and panic is likely to have a bad effect on his outlook in the long run. We do know that with young children a separation experience can often make them lose their heads. Children cry for three or four days on end; they become quite frantic and desperate. The separation situation can give rise to such overwhelming anxiety that I would be very surprised if it were advantageous. But until we have done a great deal more research, we certainly cannot answer the question.

Mlle. Inhelder asked what are the critical ages. There again, of course, we are very ignorant, but it certainly appears that the most dangerous are under 4 or 5 years. Such evidence as we have suggests that children of 6 or 7 are more able to maintain a time-perspective, and they do not seem to suffer anything like the same degree of damage as the very young children. But just where it is in the first 3 or 4 years that the most damage is done, we have no idea.

LORENZ:

Laura had a very good time-perspective, and she was particularly good at taking a grip on herself. We saw her *not* losing her head all the time. It occurred to me all the time whilst looking at Laura that she would have been damaged more if she had been slightly less intelligent than she was. This is quite in keeping with what I want to bring forward: you know that dogs are also damaged by being put in a dogs' hospital; you know that the most sensitive and the most trusting dogs refuse to recognize their masters when they come to fetch them. The most dramatic thing is described by Thomas Mann in his book *Herr und Hund*. That is a very beautiful observation on his dog when it was brought to hospital; it describes how the dog would not believe that it was going to be taken out again, and only when he really realized it, did he get back his whole trust and shake off the effect—well, actually he did not quite; he had serious after-effects. The film brought up the question, what really is the trauma

in separation? Do you not think that in this child, and the dog, the main trauma may lie in the fact that the mother suddenly and unexpectedly ceases to defend her, that mother allows her to be carried away? That is totally unsuspected from the emotional, instinctive point of view of the child. If mother were a chimp she would start fighting the hospital sister tooth and claw. I think the real trauma is the disappointment in mother all the time. I am quite sure that in the dog who is put into hospital the disappointment in master failing to defend is the real trauma.

I quite realize that this is only one factor, but I wonder whether Bowlby would agree that this is one of the major ones?

BOWLBY:

We certainly do think that the changed attitude towards the mother is a principal part of the damage that can be done, particularly the child seeing the mother as a bad lady instead of a good lady; one of the characteristic things which you get after separation, particularly a more prolonged separation than this one, is intense hatred of the mother. One of the ways in which acute ambivalence towards the mother is created in a child who loves and is attached to her mother is through her having an experience of a semi-long separation and developing intense and violent hatred towards her. Thenceforward she oscillates between hatred and love.

MELIN:

Would she not have been better off if she had not been brought up in that atmosphere of, 'Don't cry'. The whole time you see her starting to cry and suddenly ceasing again. Mother said 'Don't cry' when she came and went. If Laura could have cried a bit more and could have gone to the nurses more with her sorrow it would all have meant less to her.

BOWLBY:

I think the extent to which this child has been taught to control the expression of her feelings certainly puts a special strain on her. We know from other experience that, if a person is unable to cry at the appropriate moment, they are apt to do it symbolically or in other ways over a long period subsequently. It becomes a vicious circle. It is possible that some of the nose-picking which Laura shows is an alternative to crying, is in fact a displacement-activity. The natural thing would be to cry, but as it is she stays put and picks.

LORENZ:

Yes.

GREY WALTER:

Bowlby drew an analogy between the conditions he is dealing with and those of infectious disease, which might be a quite helpful analogy, since the question of prophylaxis arises immediately. As I understand infectious diseases, one of the first important principles which was established as a prelude to public-health control of infectious diseases was the occurrence of natural immunity and the establishment of artificial immunity. That seems an important aspect of this problem too. There is every evidence that some children have a natural immunity to this particular sort of stress; there are some children who appear to survive unscathed whereas other children would be badly hurt. Somebody sometime must engage in long-term statistical surveys to determine why this should be so. This country and other countries cannot afford ever to give to each child who may require it hospital treatment, or to coddle children to whom some social catastrophe may occur. Therefore, we shall have to select, sooner or later, those who require the safeguard of inoculation or some preventive treatment. The identification at an early age by some simple methods of individuals likely to have natural immunity to this condition seems to be the first step towards the administration of a public-health service in relation to psychiatric disorders.

BOWLBY:

This is certainly a very important step, and it involves us in trying to get some understanding of the psychopathology and dynamics of what is happening. Fortunately, however, there is also a very much simpler solution—that is to avoid all sorts of quite easily avoidable separations. I get the impression that between 70 and 80 per cent. of the separations which occur today can quite easily be avoided by ordinary, practical measures without any great expense. That seems to me a simple first step which can be taken, and is being taken, before we go on to the other problem, which I fully agree is very important, but going to take a long time to work out.

GREY WALTER:

It certainly involves a very well-equipped, very patient, team of investigators working over a long period of time. What you were saying about the practical steps that you can advise now is perfectly true of society as it is at the moment, but we must face the possibility

that conditions will not always be as good as now. We must contrive a society which will combine the plasticity of a less highly organized society with the facilities of the present society. The chance of the child being separated by accident from its parents is very small, but only a few years ago we saw very dramatically the situation in which a great many children were separated from their parents.

BOWLBY:

Not the under-fives.

GREY WALTER:

True, but the general organisation of society on an emergency basis, which we might have to arrange for again, would be much easier if we knew more about the distribution of this character in the population.

HARGREAVES:

I should like to make a comment on public-health practice. I think, historically, the intrapersonal aspects of the problem of immunology come rather late. If everything that Snow had written about cholera a hundred years ago had been applied, cholera would have been abolished before the vibrio was discovered. He stopped the cholera epidemic by taking the handle off the Broad Street pump; he had become convinced that cholera was something to do with water. I think the first step is very much as Bowlby said, to attempt to reduce the frequency of separation, because a very high proportion of separations in our society are what you might describe as elective separations. For instance, one comment that I think one would immediately make about this film is, 'Why the hell was the child put in hospital?' It is a very simple operation, which can be done in the out-patient department, and the child could perfectly well be managed at home.

Then there are a whole series of social aspects of separation. Take the nature of our income tax legislation, in which the allowance for children is so inadequate that there is a very strong pressure on the mother to go out to work. National policy has been encouraging this, in that you can put your baby in a day nursery provided you work in a factory all day, but you are not allowed to put it in a day nursery to go shopping for an hour. The aim of the day nursery is to get the mother to go and work in a factory. There are a lot of general measures that one could take now; although I entirely agree one does need to know something about those who can undergo the experience without much result and those who show strain.

MEAD :

I do not dispute the importance of reducing the role of the hospital as much as possible and creating facilities to permit the mother to care for her child much better than she now can. But there still remains another possibility if instead of always looking at the trauma we also looked at the child who had an immunity. We consistently do not do that, because almost all our skills are focussed on studying the traumatized child. There is as good evidence from comparative studies of society that it is possible to make children able to tolerate separation much more easily, because they trust more people. That is a simple thing to do, though it also requires a large number of social measures. It requires new styles of neighbourhood behaviour, different sorts of community nurseries, and different types of organization. It requires different kinds of housing for grandparents, and a whole series of social measures that fall within the field of public health, which would make it possible for at least a very large number of children to learn that there is more than one person whom they can trust.

We are at present in danger, by emphasizing the importance the mother has for the child when the child has no one else, of increasing the anxiety about separation and making mothers afraid to go out. In some places in the middle classes women are going to have to be psychoanalysed because they have left their children for two days. We are in serious danger of accentuating the situation which lowers the child's immunity to the accidents it may encounter.

BINDRA :

What Dr. Mead has said so eloquently is what I was trying to get at the other day when I was discussing our dog studies. The implication of all the studies was that you can bring up a dog normally without attachment to the mother, and that there may be ways of doing the same in the case of human children.

MEAD :

I want to make it clear, I am not talking about reducing the possible attachment to the mother but getting rid of the fact that she is the *only* person the child can trust.

LORENZ :

I knew that was what Dr. Mead was going to say before she said it, and also Dr. Bindra; that is, of course, trust in other people must be promoted, but at the same time—with dogs—you must be very careful about that, because you will get a perfectly despicable

dog very easily if you make him trust *too many* people. I am particularly anxious to make my dog distrust other people as much as I can. It may be a utilitarian point of view that it is good to have a dog that bites other people and loves you, but I confess I like dogs to be like that. This trust in others must cost something; it is not free of charge.

INHELDER:

I agree with Dr. Mead that we must give children the possibility of increasing their social relationships. Particularly in the hospital, contact with their playfellows—certainly without replacing contact with the mother—can help the sick child to adapt to its situation. The study of the bonds established very early between children has been rather neglected. I think their role in development is considerable.

FREMONT-SMITH:

We saw this, in fact, in the film. We saw Laura, did we not, entering into sympathetic relations with other crying children, which not only might have helped them, but did something for her.

There is one other point it seems Dr. Lorenz has touched on here which may involve species differences, and that is the degree to which an animal of one species needs a single or small number of parental figures, and the degree to which it can spread this over ten or fifteen persons—or whether it needs a hierarchy: one, a mother and then five uncles and aunts or grandparents. It seems to me this is again something which needs statistical testing both with respect to the species and with respect to whether or not you would make a characterless child if he has fifteen equal parents as opposed to one.

LORENZ:

I think you have to fix the number of parent-figures. I am quite prepared to believe that the child can be taught to love fifteen people and yet be quite discriminative about what kind of people to love. You have the same thing in dogs; you have family dogs who love quite a number of people and yet they are quite exclusive with strangers.

FREMONT-SMITH:

Do they not have one master?

LORENZ:

Not necessarily.

GREY WALTER:

The way in which dogs respond to separation seems to me to depend very much on the type of dog, in the Pavlovian sense. I have no experience with pure-bred dogs, only with mongrels, and there it is quite noticeable that the dogs which arouse the most sympathy and affection in human beings are the melancholic and choleric types, according to Pavlovian terminology, and that includes more than half of all dogs. Those are both extremely sensitive to separation and deprivation, and they tend to work only for one person and to resent any doubt in a situation. They require enormous reassurance and persuasion that the situation is a good one. In a sense they do not really trust their master or their friends enough. If they are taken to hospital they do not say, 'Because my master says it is all right, it is all right whatever happens.' The much rarer type of dog, the type Pavlov called sanguine, is just as nice a dog in the sense that he is capable of forming a close attachment to you, but his relationship to you is not so much of the slave and master as of the colleague. He will not feel he has got to work just for you, he will work with you. He will take a tough situation, such as I described in the conditioning experiment, and work it out and retain almost complete immunity. Although it is a rare type it can be distinguished by the rule-of-thumb methods of the Pavlovians, and it is that sort of process I want to suggest is possible for humans. Obviously the situation is likely to be very much more complicated and there will be more factors to be considered, but there is a possibility that some such typology may exist in human beings.

BOWLBY:

When one talks about separation one is talking about the child being removed from his familiars to a situation where he is cared for by strangers. All sorts of arrangements whereby grandmothers, aunts and uncles are brought in as aids are obviously desirable, but we have got to be careful about the extent to which we do it. For instance, to go back to Laura for a moment, we had a striking episode occur five months after the film was made. The mother had a new baby; she went to hospital and because of complications she was there for five weeks. Laura was looked after by her grandmother whom she knew. The father, for some reason which is not very clear, felt it would be a good thing if he did not visit them.

FREMONT-SMITH:

The grandmother and Laura?

BOWLBY:

Exactly. Now, at the end of the five weeks the mother returned home and this was the course of events. First of all mother 'phoned grandmother and said, 'I'm home now, bring Laura round' and she spoke to Laura on the 'phone. Laura was very excited to hear her mother's voice and very eager to get home. The next step was that they returned home and Laura continued very excited. She opened the door and there was her mother. At this point she went completely blank and after a lapse of time said 'But I want my mummy'. For the next forty-eight hours she failed to accept this woman as her mother.

LORENZ:

She looked at her mother and said, 'I want my mummy'?

FREMONT-SMITH:

In other words: 'You are not my mother'.

GREY WALTER:

Of course, she was a different shape from when she had seen her last.

BOWLBY:

For forty-eight hours she failed to accept the woman as her mother. On the other hand, she accepted her father virtually instantaneously as far as we can make out and was fully oriented to everything else in the house; but the mother was 'washed out'. Now we do not think this was due to pregnancy, because we have got so many other anecdotal stories of exactly this kind from all sorts of other quarters and in other circumstances. This failure to recognize, a failure to accept and respond to, is not uncommon. Of course, there are several things going on here which need disentangling. Some children appear to recognize but cannot respond, and the failure to respond may last a few hours, a few days, even a week. By failure to respond I mean that they do not treat the woman as their mother and do not adopt a 'following response' to her. At a certain point this phase breaks suddenly—it is usually accompanied by tears and protestations—and from that point onward the child glues itself to his mother and remains thus for some weeks or months. The particular thing which Laura showed is just one of many possible reunion responses—about which we still know too little.

TANNER:

Do you suppose that this is related to the previous experience you filmed, or do you think that this occurs to some extent anyway -- as a result of that second separation -- though perhaps not so dramatically as in this case?

BOWLBY:

It is impossible to answer that question; we have not unravelled it sufficiently.

FREMONT-SMITH:

It seems to me not an uncommon experience for parents to be reunited with a child whom they have not seen for some time, and to find the child really is strange. I think, and this may even be true with adults, that part of it is a question of the image that you have developed or carried of the loved one in their absence, and sometimes, I think with a child at least, one has regressed in the sense of going back to the image of the child at an earlier date; one has lost the most recent image of the child.

LORENZ:

May I interrupt with a story; it applies to the word image? It is a story of my little daughter whom I left when she was three and did not see again for about seven months to a year. She loved me very dearly and when we met again she seemed somewhat reserved, but she greeted me very politely and nicely and was very glad to see me. Then when I had left the room, very tactfully so that I could not hear her, she said to my wife, 'But our former Daddy was so much more beautiful'.

TANNER:

I have seen exactly the same thing with adults when the prisoner-of-war returns after being separated.

STRUTHERS:

The points I was going to bring up have already been mentioned, particularly by Dr. Hargreaves and Dr. Mead. One of the things which disturbs me is the very casual use of hospital beds mentioned by Dr. Hargreaves. I am thinking particularly of the most recent edifice erected in America for the study of the diseases of childhood at a cost of \$15 million. My own belief is that if they had put \$5 million into the hospital and taken the interest on \$10 million to keep children out of hospital by providing home services, the community as a social body would have been very much better off.

Dr. Hargreaves' point, why the hell was this child put in hospital for something that could have been done in the Outpatients, is extremely well taken. It probably would have taken a surgeon about twenty minutes to put a subcutaneous purse-string suture in the child with the umbilical hernia; the child would have been home in four hours, and the separation trauma from the mother would have been completely avoided. It applies to a great many operations and conditions for which we admit children to hospital supposedly for purposes of study, which could have been done outside the hospital—probably with somewhat more trouble to the physician or surgeon or whoever is interested—with a lot of the trauma being saved. I think Dr. Bowlby's film should be shown as widely as possible as a propaganda film for the education of young doctors in surgery and medicine; because they need it.

I would like to ask Dr. Mead if I am correct about one point. It would appear to me that the higher the family gets in the grades of civilization or in social status, the fewer the number of people in whom the child comes to trust; and the higher you get in the social scale, the more the child is protected from contact with other individuals whom he should learn to trust. Is that true?

MEAD:

No, I would not think that is so exactly. In the upper classes in many societies you have large groups of servants, gardeners, and so on, and you have a similarity in character structure between the upper-class child and the peasant child, because in both instances they have a fairly wide and varied group of trustworthy persons who care for them. It is particularly in the middle classes,—in a society like Britain from the upper working class through the lower middle class (and this is now being extended into the upper middle class because servants are so expensive),—that you tend to get the small house, the exacting standard of living, the keeping oneself to oneself, the mother sufficiently free from very heavy outdoor labour or factory labour or anything of that sort and also pretty free from a very elaborate social life, and that concentrated relationship between mother and child which is one of the things which produces what we call the lower-middle-class character.

ZAZZO:

In connection with the dangers of exclusive attachment to a single person I call to mind twins with whom I have been concerned for about ten years. Only on two occasions were the twins separated from their mother. It was at about one year and one-and-a-half years. There was no hospitalism or separation reaction. On the

other hand, every time—and this occurred frequently—that I studied the separation of twins who were terribly attached to each other I observed disturbances very similar to those you have described. Here it is a case of an exclusive attachment of identical twins brought up together—an exclusive attachment which is obviously very bad.

FREMONT-SMITH:

It seems to me that the attitude and the understanding of the nursery staff and of the doctors in the hospital is something which could be given consideration. There is no reason why the hospital staff should be as un-understanding of what the child is going through, of what the child's expectations are; and I should assume that we could develop some rules-of-thumb which would make it possible for the nursing staff and the medical staff to ameliorate the misery and trauma of the separation where such separation is necessary.

HARGREAVES:

Could I commend the work of Lester Coleman in New York, who is an ear nose and throat surgeon who tackles this problem with great care. He sets out to become one of these associate parent figures and plays the role of carrying the child through. He insists that the child comes to see him two or three times before the operation, in his office. He plays with them, tells them all about the hospital and what is going to happen. They play with the anaesthetic apparatus. In the hospital he says he never gets the elevator, he always fetches the child himself from the ward, because it gives him time to talk while they walk upstairs. You see in his film the child walking up the stairs in her nightgown holding the doctor's hand. 'And,' he says 'in a minute we will come to the critical question.' Always during this stage the child brings up her greatest anxiety, usually anxiety about unconsciousness. 'And shall I wake up?' He sits down on the stairs and talks about this before they go on to the operating theatre; he is prepared to talk about it for five minutes. I think he has shown quite clearly, not only the need for the child of substitute parent-figures, but that the medical staff, if they take the trouble, can get into that role in time to help the child through.

BOWLBY:

Yes, this is true and valuable for older children, but it is very doubtful how much can be done for the very young child, say under three. It is always difficult for people to realize the great limitations in these matters of the very young child.

SIXTH DISCUSSION

Presentation of Film by Dr. Lorenz

LORENZ:

What I want to show you is an immature film,* partly meaning that it is about immature humans, and partly that the film itself is immature, because the maturation of what we call ethograms, meaning inventories of behaviour patterns, is incomplete, and this sort of endeavour is doomed never to attain complete accomplishment. We will see a number of behaviour patterns of prematures, newborns and infants up to about one year of age.

(Dr. Lorenz showed his film.)

We concentrated on spontaneous behaviour; we left out the extra-pyramidal reactions and others that have been very extensively studied already, and we tried to put a special accent on maturation, and particularly on the maturation of afferent control of spontaneous activity. This growth of afferent control is very essential for our conception of the reflex. We do not believe there is any sharp boundary line between what is generally called reflex and spontaneous nervous activity. As an example let me take the development of what is termed by Peiper (1949) 'der orale Greifreflex', or 'the oral prehensile reflex'. It is released by touching the infant's cheek and it consists in the infant's turning the head and snapping for the finger.

What happens in maturation is this: the new-born kitten, rat or child has what Prechtl (Prechtl and Schleidt, 1950, 1951) calls the breast-seeking movement of turning the head rhythmically from one side to the other. You will see it occurring spontaneously in a premature for about 14 days after birth; a short time afterwards it becomes inhibited by higher centres. At the period when it is not yet releasable by touch it is already inhibited, so that for a time it does not occur. After that it becomes releasable by touch on the cheek, and it is stopped by getting hold of the teat.

In the cat, the interaction of endogenous spontaneous movement

* PRECHTL, H. F. R. *Reifen der Frühkindlichen Motorik*, Film aus dem Institut für den Wissenschaftlichen Film, Göttingen.

and afferent control is still more beautiful. Here the movement is not stopped by the finding of the teat, but by finding the hairless area round the teat.

In its final state the 'oral prehensile reflex' is indeed a clear example of a reflex. But its ontogeny shows very clearly that spontaneous automatic movement plays one part, central inhibition another, and afferent control of that inhibition a third. The oral prehensile reflex consists of: (a) one process of afferent control releasing the automatism from its central inhibition, (b) the performance of one quarter of the full phase of the rhythmical and automatic to-and-fro swinging of the head, and (c) the re-instatement of central inhibition by another process of afferent control. In ontogeny, the endogenous movement appears first, then the central inhibition seems to follow, while the processes of afferent control evidently come last.

Something very similar seems to hold true for other reflexes as well. Precht (1953) has shown that the gripping reflex tends to show a rhythmical and automatic repetition in prematures. So does the sinew reflex, under certain circumstances, and many others.

A second example concerns the Babinski reflex. To the question 'does a normal adult man have a Babinski?' the accepted answer is no, or at least that this reflex is inhibited by the control of the Pyramidal system. Yet, as you will see in the film, Babinski's reflex appears quite normally in adults, only not independently by itself but woven into a context of other movements: those of walking. The typical movement of the Babinski is released every time the foot is put down, at the moment the heel touches ground. When the sole is pressed down, the Babinski is followed by the so-called gripping reflex. You will see that in the film, it is always Babinski-grip, Babinski-grip. As the afferent control matures, the Babinski becomes unreleasable by such simple stimuli as evoked it at first and, from now on, occurs only in the context of walking, with all the superimposed mechanism of learned movements. Adult man still possesses—and uses—the mechanism and the movement of the Babinski and what we call 'a Babinski sign' in the sense of a pathological symptom is just this mechanism minus the complicated nervous apparatus normally controlling it.

GREY WALTER:

Why is the Babinski released by a pyramidal lesion?

LORENZ:

Well, maybe the pyramidal functions take over part of the movement which originally was brought about by the Babinski.

GREY WALTER:

That is rather far-fetched, I think.

LORENZ:

What Grey Walter means is that in pyramidal lesions you get failure of motor function and not of afferent control. I have no answer to that, it may be a serious objection.

MONNIER:

The most important new result of this film is certainly the relationship between Babinski and the grasp.

LORENZ:

I think so too.

MONNIER:

I have a slightly different interpretation of the sign of Babinski. The sign of Babinski is a part of a nociceptive flexion relation. We have many proofs of that.

The observations of Fulton (1943) in higher primates point to a synergy of flexor pattern with the purpose of avoiding the nociceptive stimulus. These nociceptive patterns are normally under the control of area four. Removal of this area four releases the nociceptive reflexes, described by Babinski, Shaddock, Oppenheim; they are all selectively controlled by area four, and by area four alone. In a more frontal region another centre, area six, controls the grasp.

There is a sign homologous to the sign of Babinski, but in the grasp series, the so-called sign of Rossolimo, which is controlled by another centre. You get it tapping the fourth toe from below; then a grasping reaction occurs. It seems to me that both mechanisms, the nociceptive response called Babinski and the grasping reflex called Rossolimo, are well demonstrated in the film.

LORENZ:

I did not quite dare to mention it, but it was my conviction that the Rossolimo does play a part in this whole series. It is not simply Babinski-grip, but Babinski-Rossolimo-grip with every putting down of the foot. One element of the Rossolimo in every grasp is the spreading of the toes. I think that this element of spreading the toes before grasping can be noticed several times in the film.

MONNIER :

It was very typical, but spreading the toes belongs still to the nociceptive Babinski series, not to the Rossolimo grasping series.

The other interesting thing is that when analysing the functions of these cortical centres—area four, area six—one gets the impression that altogether these centres control the activity of a normally acting and working human being, called ‘*homo faber*’ (Monnier, 1946, 1948). In a pure pyramidal syndrome—they are very rare—after destruction of the pyramidal tract by a shell splinter, we find only spasticity of the fingers and hand in pronation and extension; that is all. In the legs there is no spasticity whatever, but increased nociceptive flexor responses, called ‘*triple retrait*’ by the French. There is always very strong flexion in the hip and a tendency to dorsal flexion in the great toe. If we try voluntarily to flex one leg we will automatically flex dorsally the great toe and the foot. This flexor synergy is certainly predetermined, and has a nociceptive function. In physiological terms, this means that the cortical pyramidal centre controls defensive flexor reactions in the legs in order to maintain standing extensor posture, and controls extensor tone in the fingers in order to keep the hand moving freely in the ‘*homo faber*’. The role of this cortical centre (area four) consists in increasing the motility of the hands, of the feet, and to protect at the same time visceral organs such as the intestines (abdominal reflexes) and the testicles (cremaster reflexes).

As an additional function I should mention vasodilatation of the fingers induced by area four. Thus mobilization of the fingers with vasodilatation is a function of area four which controls also the postural tone of the fingers. It would be very bothering if this tone would not be partially inhibited during skilled movements. That is why I call the cortical pyramidal centre in area four a centre for the ‘*homo faber*’.

LORENZ :

It frees the hand from too much postural tone.

MONNIER :

Yes. This postural tone must be controlled electively; it will normally be increased in the shoulder, arm and legs, and diminished at the same time in the fingers. The extensor tone occurs only in the proximal part of the arms and in the legs under normal conditions, while the distal parts (fingers) show on the contrary a decrease in postural tone during voluntary movements. The functional purpose

of the pyramidal system is understandable only if the behaviour of the whole individual in activity is considered.

Another interesting feature of the film is the occurrence of a grasping posture in the hands while the baby is sucking the milk bottle. We know in neurology an interesting association between the little muscle called *musculus quadratus* in the chin, which has something to do with the expression of the child during sucking, and the palm of the hand. Scratching the palm of the hand in young children induces a little contracting of the chin; this is called 'réflexe palmo-mentonnier'. It is an example of a synergy between hand and mouth.

GREY WALTER:

The interesting thing is, this is the only reflex of the body which can be excited by a single stimulus, a single electric shock. It is the perfect reflex for the study of the spino-cranial reflex in man for this reason.

TANNER:

At what age does it disappear?

MONNIER:

During the first year.

GREY WALTER:

But does it not persist in about 30 per cent. of the adults?

MONNIER:

Yes, and also in degenerative diseases of the brain—such as the 'scléreuse tubéreuse de Bourneville'.

The last thing I wanted to say concerns the anencephalic newborns. The rhombencephalic type of anencephaly shows only the grasp and flexor patterns; extensor patterns are very rare, such as the stretching of the arms as a late component of the Moro-reaction, with the purpose of expanding the basis of the body. It belongs, I think, to the anti-gravity functions and shows a certain rivalry with the Babinski and the grasping reactions. This is also very interesting in your film. I observed the same rivalry between sucking and the nociceptive withdrawal of the head in the period during which elementary mechanisms are not yet controlled by higher centres.

GREY WALTER:

I have a small contribution on this very fundamental question of the growth of afferent control of the nervous system. An example of

this was given by Grossman (1954). He was studying the development of electrical activity, and particularly evoked electrical activity, in small animals. The response to any stimulus in any receptor modality in the very young creature seems to be a generalized response of the brain. One tends to think of the new-born baby as being a very well organized sort of spinal reflex system with a lot of functionless jelly on top, but that is not quite a correct view, it would seem. It looks as if the jelly were working fairly well so as to generalize the responses. A great many of the responses which were shown in the film are the effect of rapid diffusion of activity throughout the upper part of the neuraxis modifying the spinal reflexes even from before birth. You get these violent, extensive and reflexive movements which are quite reasonably called pseudo-convulsive movements. This is the standard, normal, basic element of central response in young creatures.

FREMONT-SMITH :

Do we know in mammals or humans whether there is any motor movement prior to sensory inflow?

GREY WALTER :

It is a difficult experiment to do.

LORENZ :

I think it is possible to do the experiment. The common-sense assumption is that it will be somewhere between the two extremes of total reflex and total spontaneity. I agree that all these spontaneous movements need sensory 'loading up' like Gray's and Lissman's dogfish in which a certain amount of afferent input was necessary for the loading up of the system. But I am quite sure that the co-ordination of the to-and-fro movement is central correlation independent from proprioception and afferent or cortical control.

GREY WALTER :

I wonder if that is true. It is a difficult experiment to confirm; I am not quite as convinced as you are. I feel there is a great deal of stimulation going on in practically all modalities under conditions such as in your film. The immature cerebral regions of such animals as man have a capacity for rapid diffusion of response to stimulation. That being so, it might be more likely that there would be survival value in the capacity for amplifying the effects of small centres of

stimulation which would gain more specialized effects later on as the growing animal became more experienced.

RÉMOND:

Even in the adult, the effect of the first stimulus is extremely generalized.

GREY WALTER:

But not the effect of the second.

LORENZ:

I think the only possibility of deciding to what extent this is spontaneous or independent is to watch anencephali. Did any of Monnier's or Gamper's anencephali show the rhythmic movement?

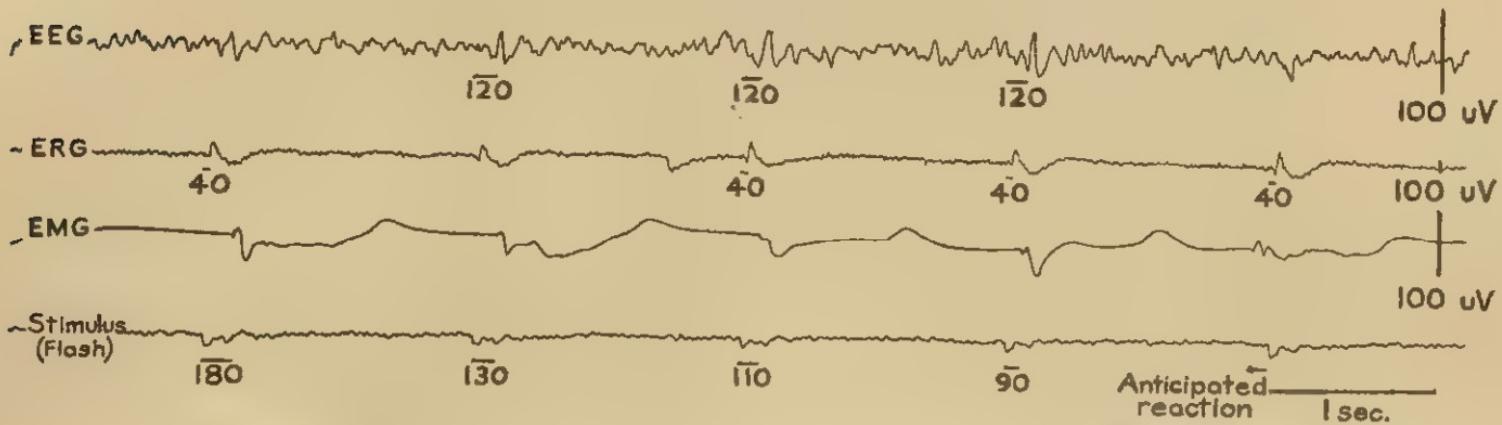
MONNIER:

The bulbo-spinal anencephali show very little spontaneous activity. If there is some rhombencephalon in addition to the medulla and spinal cord, the responsiveness to all kinds of stimuli is greater, but no important spontaneous activity occurs. The new-born rhombencephalic anencephalic reacts at once by a mass reflex to various kinds of stimuli, but chiefly to the shaking of the ground on which it lies. We may induce also a Moro-reflex on shaking of the ground.

I would like to contribute some remarks about the possibilities of measuring the learning process by electrophysiological methods in man. The technique we used was as follows. (Monnier, 1952): a subject had to answer to a stimulus of light (flash) by a movement of the finger. The retinal response to the flash was recorded by electro-retinography (E.R.G.). The cortical response in the neighbourhood of the visual centre was seen on the electroencephalogram (E.E.G.), and the response from the muscle itself by means of electromyography (E.M.G.). This allowed us to measure the motor-reaction time of the subject (Fig. 20). The subject had already had twenty tests before this record. I have chosen this part of the experiment to show what occurs in a subject learning by repetition.

The motor-reaction time shortens progressively: 180, 130, 110 and 90; it becomes shorter and shorter until finally an anticipated response is given. In other words, towards the end of a training or learning experiment the results become better; this is probably due to better attention, and ends with anticipation.

FIG. 20
ELECTRICAL RESPONSES OF THE RETINA (ERG), OCCIPITAL CORTEX (EEG) AND M. FLEXOR
DIGITI (EMG) TO REPETITIVE FLASHES



Progressive shortening of the motor reaction (conditional reaction) leading to an anticipated motor response

The latency of the retinal and cortical occipital responses shows little variation

The process of conditioning and learning occurs between the receptive cortical area and the motor centre

Measurement of the opto-motor cortical integration time may be a good parameter for testing conditioning and learning processes

spontaneously but it still took 75 milliseconds and merely started sooner; it seems to me this could possibly be an interpretation of the fact that actually you end up with an anticipated reaction which precedes the stimulus. You might argue that it had still taken 75 milliseconds in the associative centre, but it had not needed any retinal stimulus to get it there.

MONNIER :

I agree with your interpretation. This would prove that the chief variation occurs between the visual and motor centre, or perhaps in the motor centre alone.

BINDRA :

Yes, but it will not get rid of the objection that has been raised by Dr. Fremont-Smith. There still may not be any change in the crucial interval of 75 milliseconds; the reduction in reaction time may be an artefact of the method.

MONNIER :

I know that in this associative and motor part of the process, the shortening is due chiefly to the attention of the subject. We have experiments which prove that, when the subject begins to be fatigued, the motor reaction shows progressive lengthening.

FREMONT-SMITH :

But in the second one, where the fatigue comes in, it seems to me you ought to be able to show quite clearly that it does take place in the associative centres. It would seem to me that you have not yet answered my objection to claiming that the shortening took place in the associative centres, because supposing a stimulus arising other than in the retina anticipates the signal, you might still come out with a progressively earlier response. Let us take the one at 90. Supposing I were to say that this is merely the result of an anticipated response which did not start soon enough to give you a reaction in advance of the stimulus, what then?

MONNIER :

I agree, but I can give you another interpretation. I mean now that something that has been learned in the sequence of twenty responses to twenty flashes becomes, by means of attention and possibly of some will, more and more trained, and ends in an anticipation which has nothing more to do with the stimulus.

BINDRA:

That is probably the correct explanation; but I think, in order for you to make your argument decisive, you still have to exclude the possibility that a person actually starts responding before he receives the stimulus, especially as your stimuli are given at such regular intervals. The way to control this factor would be to trick your subject by interpolating some stimulus that is not the stimulus to which he is supposed to respond. If you put enough of these fake stimuli in I think you could make sure that the subject actually sees the stimuli before he makes the response.

FREMONT-SMITH:

Or you could see that the stimulus came at random intervals instead of regularly.

GREY WALTER:

I should like to say I do not think the mechanism is in any way as you described it.

MONNIER:

I should be very glad to hear another interpretation.

GREY WALTER:

There are all sorts of objections to your interpretation, but I should like to mention two pieces of work which are quite relevant; one is by Pampiglione (1953) at the Maudsley Hospital, on a rather similar experiment during sleepy states, where the completion of a pattern by the sleepy or sleeping subject is the most remarkable feature. In these experiments he evoked the well-known K complex of the E.E.G. by a non-specific stimulus in a rhythmic fashion every ten seconds, then he started to leave out the stimulus and, sure enough, for a while, every ten seconds the K complex appeared even when the stimulus was missing. Even in the sleeping subject an escapement is started which begins to count the time and when the time comes for the stimulus it begins to be anticipatory and gives a response. This response to time is the same thing that Dr. Liddell was talking about, and it involves a much more complex mechanism than you have implied there. There are more involved structures which you have not delineated which do not go up through the cortex but down, into the depths of the brain again.

MONNIER :

I agree that the optomotor associative process may be not only cortical, but cortico-thalamic and thalamo-cortical.

GREY WALTER :

It is the anticipatory reaction which really makes hay of any attempt to measure latencies, because the brain has built up its own idea of when it is going to respond and is not simply responding as a telephone exchange any more; it has built up an anticipatory image of the situation and it is going to respond to that.

Hick (1950) in Cambridge, has published a very interesting paper on the relation between reaction-times and the number of alternatives in choice-reaction situations. The results follow the expected rules of information theory as to the information being related to the number of possible alternatives. I think that is a much more profitable way of approaching this problem, because if you randomize the temporal order, or perhaps introduce some subtle temporal pattern, you begin to see which things you can measure as physiological latencies and which are measures of the cerebral interpretation and storage of pattern.

MONNIER :

I entirely agree. These experiments were done to study the relations between afferent and efferent impulses, and not with the particular purpose of studying learning, but since we met variations of the motor time according to repetitive processes, I considered it worth-while discussing this problem with you, hoping to get new working hypotheses.

GREY WALTER :

One of the difficulties is that the reaction-time experiment looks so simple. 'When you see the light press a key' sounds simple, but actually it is very complicated and involves a number of preconceived conditionings. There is associative learning there in a sense, though very little, because you have preselected his association for him; you have said, 'associate this only with the light'. There is also practice. In your choice-reaction experiment, however, you begin to get something that, although it looks more complicated, is in a sense simpler, or more analysable. Here you have a situation where the subject has to build up an association; he has to decide between all the possible lights which light has gone on and what the correct response is to that light.

BINDRA :

It is a little too complicated if you ask a person to make a choice reaction of this type; you are making the situation more complicated than Dr. Monnier would like it to be.

GREY WALTER :

It is not actually, because the complication is analysable on the basis of information theory the moment you know what the choices are. In this case you do not know what the alternatives are.

In the response to patterns of flicker of 180 to 200 milliseconds, latency was of the same order as that which you give, between 180 and 200 milliseconds. Therefore the latency for the appearance of a cerebral response to pattern stimulation in the non-projection areas of the brain is about the same as the reaction-time.

TANNER :

Does that latency decrease during the process of conditioning?

GREY WALTER :

Yes, it decreases, and then it may increase again when the subject gets bored. You have not provided much reward here except the rather tenuous one of the subject's supposing that you are pleased. If you do that kind of experiment you will find a gradual decrease in the latency. It is not very much; but as the subject begins to get bored or becomes satiated it will increase again and the response may become generalized. In our experiments, as it happens, the boredom is more marked than the conditioning. We find we cannot give the normal human being sufficient reward without its either costing us too much or involving us too deeply with our subjects.

BINDRA :

What happened to your E.E.G. records, Dr. Monnier?

MONNIER :

You see, for instance, this huge component occurring in the E.E.G. 120 milliseconds after the flash (Fig. 20); it is related to the response of the visual and paravisual cortex. You will see after this huge response a volley of alpha waves. We may say that the optic stimulus induces first a specific response followed by the huge potential, and afterwards, very often, by three or four alpha waves as an expression of relaxation of the visual cortex, after the specific response.

GREY WALTER:

That introduces another effect of which one must beware in these responses. It is known as the 'Bates effect', because John Bates described it first, I think, (Bates, 1950). He starts with a very simple situation in which a normal subject, sitting quietly, is asked to clench his fist or make some movement just when he pleases—several times a minute, but exactly as he likes; he took a series of E.E.G. records during these trials and superimposed them, using the moment at which the subject chose to make the voluntary response as the fiducial mark. He found that just before the movement was made the alpha rhythms fall into step and that the subject makes the movement only at a certain phase of alpha rhythm, or some component of the alpha rhythm—that the moment at which he chooses to do something is determined, to some extent, by a gating mechanism inside the brain which allows him to make it only at a certain moment. Actual 'now' is not within one's free choice, so that will produce a scatter in the reaction-time—and that might have a very important effect.

HARGREAVES:

It is only when a ball bounces that you can catch it, and you cannot catch it between bounces.

BINDRA:

We have a similar finding on rats. Rats are made to cross a barrier under threat of electric shock. They are able to avoid the shock by moving from one end to the other of the grid every 10 seconds. This is a temporal conditioning situation. Then, through an electrode, we pick up the various happenings in the hypothalamus. One sees a very characteristic response just before the rat begins to move in this direction or that. Seeing this characteristic signal enables one to say, 'The rat is going to go now'. We call this characteristic hypothalamic response 'a picture of free will'.

RÉMOND:

Before a rabbit is going to move, he has a very special pattern in his E.E.G. which may not be completely different from the rat's pattern (Rémond *et al.*, 1950, 1951).

TANNER:

While he is making up his mind?

RÉMOND :

Yes, and it is a very recognizable sign; 20 to 30 milliseconds after its occurrence, the motion follows.

About ten years ago we were studying the reaction-times related to E.E.G.s, trying to learn more about the state of consciousness in which a subject might be. We found first that when the stimulus was repeated rather rhythmically the reaction followed very quickly; and sometimes there was no reaction-time—it was instantaneous. When the stimulus was given at random, the reaction-time was much longer. In patients, especially in idiopathic epileptics, the reaction-time was always the longest. But these people were able to shorten their reaction-time when the stimulus was rhythmically repeated, and I have the impression that the ratio between the response to a rhythmic stimulus and to a random stimulus was about the same in normal and in idiopathic epileptics, even if the reaction-time of the latter is ten times that of the normal.

INTERVAL

INHELDER :

Learning theories analyse in detail the experimental conditions of learning, such as the number of stimuli and their rhythm, the subject's motivation and vulnerability. One fundamental aspect of learning seems, however, to have been somewhat neglected by most of the learning theorists, and that is the psychological process by which facts are recorded, or in other terms the 'interpretation' of the experiment.

Dr. Whiting says that a theory of social learning should explain the acquisition of the cognitive structure.

Koehler and Tolman have already underlined the fact that learning implies changes in apperception and in cognition and have shown that the improvement of performance depends upon the precision of the cognitive schema which guides and directs the response process. However, their experiments were carried out on animals and human adults, and not on children. As soon as one studies the developing child one realizes that the cognitive schema which determines the interpretation of experiments is modified by elaboration of mental structures. The child of five interprets the same experimental facts differently from the child of seven or eight years; the latter interprets them differently from an adolescent of fourteen or fifteen.

M. Piaget has called this interpretation of experimental data 'an abstraction' and he distinguished two types: the physical type of

abstraction and the logico-arithmetic (or mathematical) type of abstraction (PIAGET, 1951). In these two kinds of abstraction the child is always active: he knows the external world only through his activity, whether it be sensorimotor, symbolic or rational. It is precisely these forms or schemata of activity which are transformed during growth; which explains the relation between age and the structures implied in the interpretation of experimental facts.

I should like to give you very briefly a few examples by way of illustration.

In a first experiment, which was described at our last meeting (see Vol. I), we asked children to draw or indicate by gestures how they imagine the water-level in vessels which are upright, inclined or placed horizontally. The experiment was carried out in three phases.

First phase: the child sees the vessel but not the water-level and has to imagine the position of the liquid. He can either mark a diagram given to him with a blue pencil or he can indicate his reply by gestures.

Second phase: we show the vessel uncovered in different positions and ask the child to draw the water-level as he sees it.

Third phase: we hide the water-level again and ask the child to draw it again without seeing it.

The experiment is, then, very simple and allows us to follow the learning phases which occur for an adequate interpretation of experiments (PIAGET and INHELDER, 1948). Here, briefly, are the results. They were obtained from 250 subjects of three to nine-and-a-half years.

First type of solution: the children of three-and-a-half to four years draw the water inside the vessel without abstracting any special position from the actual water vessel.

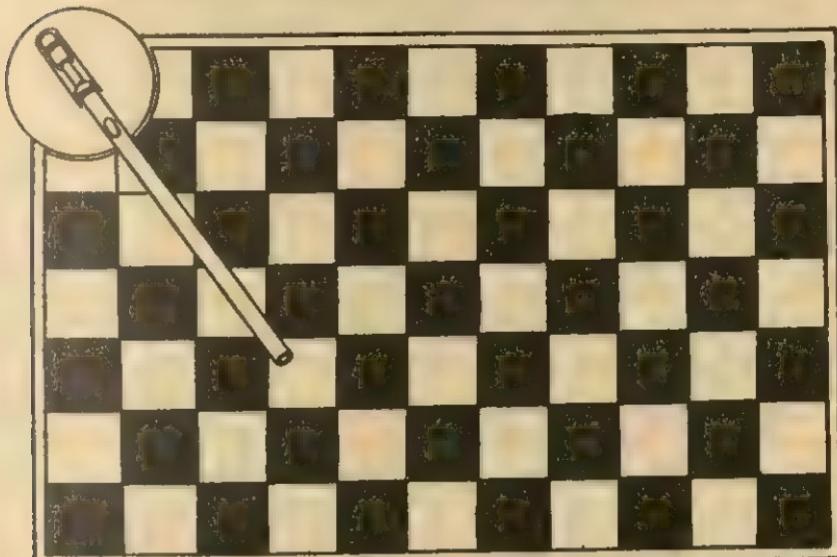
Second type of solution: the vessel and the liquid form a kind of rigid whole, which explains the drawings of the water-level in vertical, inclined or even suspended positions, results frequent at an average age of five-and-a-half years.

Third type of solution: the liquid is displaced in relation to the vessel without the horizontal position being abstracted (average age six-and-a-half years).

Success depends, of course, on the amount the vessel is inclined. For the position upside down we have already 56 per cent. correct solutions at six years and 96 per cent. at seven years, whereas for the inclined position we only get 8 per cent. correct solutions at six years, 36 per cent. at seven years, 68 per cent. at nine years and 65 per cent. at nine-and-a-half years.

It should be noted that before the age of six-and-a-half years there is hardly any learning through observation, whereas from that age

FIG. 21
EQUALITY OF ANGLES OF INCIDENCE AND REFLEXION



onwards the child gets increasingly receptive towards the experiment. It appears from these facts that in order to interpret this particular experiment in a profitable way a child must possess the specific intellectual instruments permitting him to abstract the horizontal position from the experimental data themselves.

The role of the interpretation of experimental facts, insofar as it is the result of a mental abstraction, is shown by a series of experiments intended to study inductive reasoning in 1700 subjects of five to sixteen years (INHELDER, 1954; INHELDER and PIAGET, 1955).

We wondered how children and adolescents could discover from mechanical devices the equality of the angles of incidence and reflection in a billiard game (Fig. 21). For this purpose we made several pieces of apparatus with the help of Mr. Hans Aeble and Miss Lydia Müller. By means of a cue which can be directed at different angles the child or adolescent can project a billiard ball against a deflecting wall. We asked him to reach with his ball, after deflection, a little figure in different positions and to try to draw up the rule of the game which allows him to succeed every time without preliminary experiments or trials.

Here, very roughly, are the results obtained. During the first phase (five to five-and-a-half years average age) the aim of the experiment for the young child consists essentially in finding in reality the

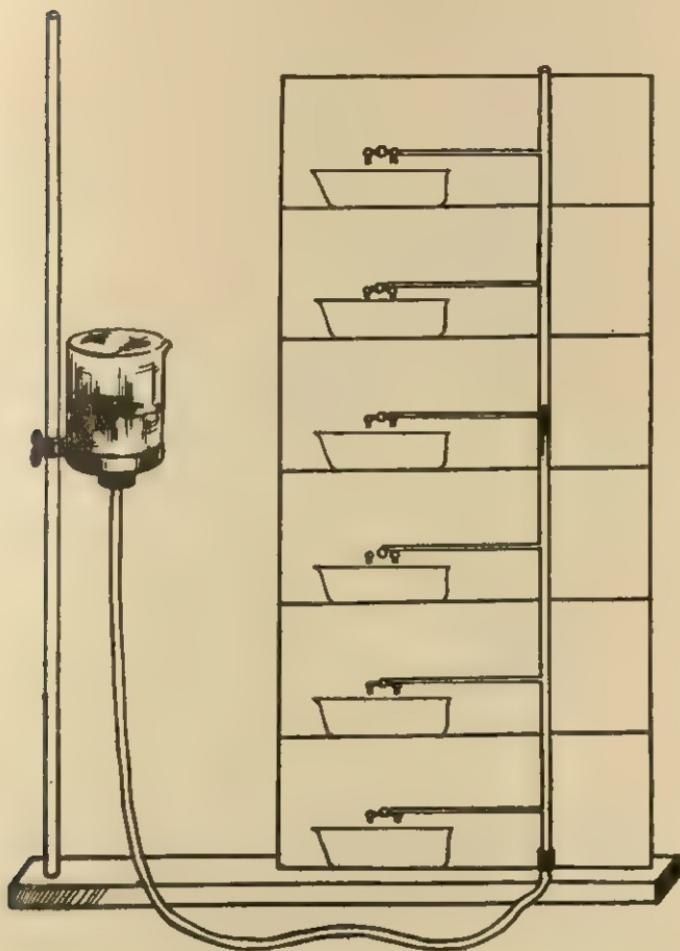
confirmation of his desires and anticipations. Certainly the child is already capable of seeing his successes and failures but he is often incapable of following the path of the billiard ball. When we ask him simply to indicate this by means of a gesture he very often draws a curved line. We have often been astonished to note to what extent young children, through lack of schemata of abstraction, remain impervious to experience. They do not yet feel any real need to compare the different results obtained.

During the second phase (the most characteristic reactions are found at an average age of nine years) the objectivity of interpretation increases. The interpretation of the experiment during this second phase is based upon a set of concrete operational schemata. Such correspondences are found by the child between the directions of incidence and reflection. The child will say, for instance, 'the more bent it is, the more it comes back bent' or 'the more I move this way, the more the billiard ball comes back the other way'. It is, of course, easier to translate the movements by gesture than to conceptualize them. It is during this second phase that the child appears to come closest to the facts; when he is younger he tends to deform them; when older he tends to go beyond them in order to integrate them into a whole system. Experimentation during the second period is, then, very productive. Thanks to operational abstraction the child can work with the apparatus although he is not yet capable of organized research.

During the third phase (the most characteristic reactions are found at fourteen to fifteen years) interpretation consists in translating concrete facts into abstract concepts. The adolescent uses geometrical systems of reference. He says, for example, 'You have to think in straight lines' or 'If you put the cue perpendicular to the cushion the billiard ball comes back on itself'. In this way, by means of a group of geometrical constructions, the adolescent manages to understand that the angle of incidence of the trajectory is equal to the angle of reflection, whatever the position of the cue.

We wondered to what we should attribute the relatively late appearance of this last form of the interpretation of experiment, since from nine years on children can generally compare angles and know the elementary geometric operations (PIAGET *et al.* 1948). However, during the second period the child is not yet capable of abstracting the equality of angles. This incapacity arises from the fact that the child does not yet try to discover an invariant. But from fourteen to fifteen years on, the adolescent is no longer content to record experimental variations: he attempts actively to isolate an invariant or a function in the mathematical sense of the term. The interpretation of the experiment seems to be always directed

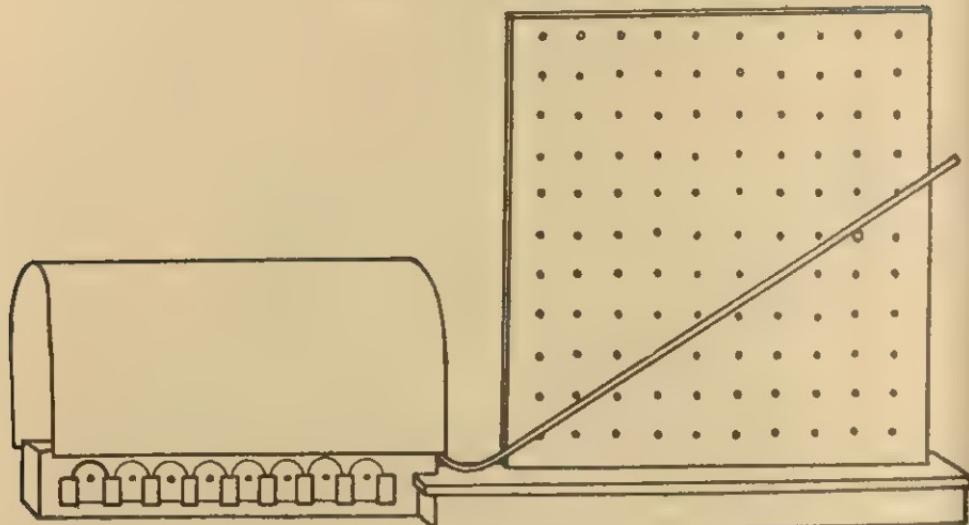
FIG. 22
WATER LEVELS IN COMMUNICATING VESSELS



by aims or goals. The abstraction is only an instrument serving the goals.

Here, briefly, is a second mechanical device consisting in communicating vessels (Fig. 22). How will a child or adolescent read the experiment, which consists in observing that in a system of communicating vessels the water-level is always at the same height in the two tubes? In our apparatus one of the branches of the system of communicating vessels is fixed and the other is movable. It is made up to imitate a dolls' house where the child can bring water up

FIG. 23
FALL OF BODIES DOWN VARIABLE INCLINE



to the different floors (with young children one can never be too concrete). The experiment was carried out in three steps: the child goes to work displacing the movable tube while the fixed pipe is first screened, then uncovered, and finally screened again.

During a first phase of development (five to seven years on average) the child who is confronted with the apparatus abstracts from his own actions. He says, 'It has to go up' or 'It has to go down'. The actions that he has carried out with the apparatus are the best remembered.

During a second phase (seven to thirteen years on average) a child observes first of all the displacement of the liquid in relation to the most immediate system of reference, which is the jar. He can already abstract the fact that the water-level is displaced in relation to the latter, but he does not yet understand that the two water-levels are always at the same height. He is still surprised by the experimental variations, whereas during the third phase (from thirteen to fourteen years onwards) he suddenly observes the equality of the water-levels. Here we find the confirmation of the same principle: the search for an experimental constant determines the interpretation of the experiment.

With the help of a third mechanical device (Fig. 23) the fall of bodies on an inclined plane is studied in a very concrete way. The child or adolescent can discover through experimentation that the

length of the trajectory of a ball is related to the height of the point of departure, whatever the mass of the ball. This physical principle is studied by means of an apparatus. The children are free to alter the inclination of a tube which serves as an inclined plane, and the balls, following a parabolic trajectory, jump over a spring-board fixed at the lower end of the tube and fall into one of a series of pigeon-holes. The balls can be retrieved from behind little doors which close the pigeon-holes.

Three ways of interpreting the experiment, each determined by specific attitudes and goals, caught our attention. The young children of five to seven were incapable of recording the experiment and reproducing it under the same conditions. Their activity is guided not by a need for objectivity but by the pleasure of setting something in motion. The child of seven to eleven is already capable of abstracting or establishing the correspondences. He discovers, for example, that 'the more the slope is inclined, or the greater the distance the ball rolls, the farther the ball will jump'. He is interested both in making some experimental variations and in reproducing the experiment, without, however, yet discovering the invariant: the height from which the ball falls.

The adolescent of fourteen to fifteen sets up the experiment quite differently. He is not only struck by the variations in the inclination of the tube and in the length of the trajectory, but also by the unvarying height of the point of departure. Moreover, by means of a systematic consideration of all the factors involved, he observes that the length of the trajectory is independent of the variations of mass.

These few facts, which are only a sample of a wide investigation on the experimental attitudes of the child and the adolescent, seem to indicate that all learning presupposes an interpretation of experimental data. This interpretation of facts or abstractions is, of course, determined by a group of emotional factors which have been made evident by the learning theorists. But it depends also on cognitive factors, such as the intellectual instruments at the child's disposal for the assimilation and interpretation of physical data, and the experimental attitudes (goals for action) which direct the investigation.

During the first phase the child acts through pleasure of setting something in motion rather than to modify the course of an experiment. This attitude conditions a subjective interpretation of the facts. During the second phase the child acts in order to produce a clearly determined, concrete result; this attitude facilitates a more objective interpretation and requires operations of comparison and of measurement. During the third phase the mechanical device becomes more and more an occasion for deduction and verification of

hypotheses. The interpretation of facts becomes an integral part of an actual intellectual construction. Thus the forms of learning cannot be isolated from the instruments of knowledge. Both undergo an evolution according to age.

PIAGET:

What is striking about these facts is that physical experiment comes much later than one would imagine. The abstraction of a horizontal line from reality would seem to be something immediate and perceptive. One would think that the baby who can walk has already some idea of the vertical when he is standing up and of the horizontal when he is lying down. Certainly he has the postural notion, but he has no perceptive notion whatever in connection with external space, and certainly no representative notion. In order to 'read' a horizontal line it is not enough to look at it, it is necessary to have the instruments which allow one to 'read' it, and here the instrument is formed by the axes of coordinates, by a system of references. A mathematical or logical apparatus is necessary if the physical experiment is to be reached. Without a mathematical or logical apparatus, there is no direct 'reading' of facts, because this apparatus is a prerequisite. Such an apparatus is derived from experience, the abstraction being taken from the action performed upon the object and not from the object itself. It is definitely based on coordination of action. It is these coordinations of action which give rise to the first abstractions, permitting the construction of logical and mathematical operations, and once these instruments have been constructed the interpretation of experimental data is entirely different from what it was before. This, I think, is the main lesson to be drawn from these facts: that physical experimentation comes much later, even in the cases where it is absolutely elementary, than logical-mathematical experimentation, and I do not think it is simply a question of complexity or facility of generalization. It is that the first experience provides instruments which are themselves necessary for a reading of the second experience: the physical experiment.

GREY WALTER:

Professor Piaget is quite correct in saying that insight into physical conditions is a very late arriving feature in human development. About ten years ago, I undertook some special lectures for schoolchildren who had to leave school and go into industry. I tried to introduce them to the principles of science, and, among other things, I presented them with the Galilean proposition of: you have a 10 lb weight and a 1 lb weight and let them go together: which will reach

the earth first? We had a democratic vote on it. Half the class voted for the heavy one; half for the light one.

FREMONT-SMITH:

None for both together?

GREY WALTER:

That did not occur to anybody.

FREMONT-SMITH:

What were their ages?

GREY WALTER:

Fifteen. At least in England, we are living in a pre-Galilean or mediaeval world, as far as the facts of physics are concerned. I think that if you took a vote in the street some people would say, 'I do not know—probably the heavy one', and others would say, 'Oh well, there is probably some catch in it—the light one'.

ZAZZO:

I have also carried out experiments of the same type as Mlle Inhelder and M. Piaget, but on adults, and at a time when our students in Paris did not yet know the details of Professor Piaget's work. I obtained rather strange results; that is to say, educated adults—if one admits that students of psychology are educated adults—failed in the same way as children. In other words, they did not seem to have acquired the concept of conservation, or in any case, seemed disturbed by the experiment. I sometimes thought that the students were laughing at me, that they were pulling my leg, but this was not at all the case.

If a very simple experiment with a glass of water was tried and they were asked: 'what must happen if sugar is put in?' their replies were somewhat baffling. When the water—or the coffee—came half way up, they considered that the sugar melted and that the level did not rise—just like the children you have been speaking about. On the other hand, they were perfectly willing to admit that if the glass, or the cup were full, and one added sugar, the water, or the coffee, would overflow.

In short, it seems obvious to me that the operations achieved by children when they reach adolescence, are very insecure and that it takes very little to make them disappear, even in the case of an adult, and even with an educated adult.

I wonder, as a matter of practical interest, whether a connection should not be established here between the failure which may be observed under disturbing experimental conditions and the well-known difficulty, experienced by schoolchildren in general, of learning and understanding mathematics.

I think that the closer we come to an attitude of abstraction and to operations, the more the affective factors intervene to make solutions less stable. In other words, the study of the pedagogy of mathematics should be directed not solely, as it often is, towards the study of factors involved in an aptitude for mathematics, but also towards the study of a certain affective equilibrium and a certain sensitivity. It is this, I think, that constitutes the obstacle preventing certain children from reaching a given level. I do not think that any special aptitudes are required for understanding mathematics up to the baccalauréat level, or even beyond that; and I think that the failures are not due to an inaptitude or a lack of special mathematical aptitude, but in a general way to affective factors.

You all know of the studies which have been carried out on the differential psychology of boys and girls. It had been thought possible to show that girls' aptitude for mathematics was less developed than boys'. I do not think that there are less developed aptitudes, but that it is the whole personality which causes school-girls on average to be less successful at mathematics, all other things being equal, than schoolboys.

GREY WALTER :

You may be quite right, but I should have said that people who are very gifted in mathematical manipulation, not merely in passing examinations, often seem to be almost crazy as personalities. I have never seen a practising mathematician who is not a very odd personality, often with overt symptoms of emotional disturbance. I should attribute the characteristic ability for mathematical abstraction to an inborn imagery factor, which may or may not be associated with certain emotional factors.

RÉMOND :

It could be the mathematics which transforms the individuals.

GREY WALTER :

No, I should say the ability or the liking for mathematics at a very early age is associated with a certain sort of psychological imagery structure, a certain type of E.E.G., too, which tends to encourage such people to become abstract mathematicians. But it may be that

a brilliant mathematician is not the same as somebody who passes a mathematical examination.

PIAGET:

If you mean individuals who produce creative mathematics, which is quite a different matter, I am entirely in agreement with you; these are special people, but Zazzo was speaking of that average comprehension which progresses to a certain degree for absolutely elementary and general mathematical operations. Here I entirely agree with him. There is no particular aptitude for mathematics and, in general, those who appear not to possess this aptitude are the victims of a simple affective blocking. In any teaching, when one does not understand anything the first month or two, one can very well get interested later on and make up for the time lost, whereas in mathematics, if a few months are lost in the development of the subject, and especially at the beginning, one very quickly gets a feeling of personal inferiority and has the impression that one can never learn anything. And therefore, on the one hand, one does not make the necessary effort, and on the other hand, as one is sometimes faced by a professor of mathematics who reasons as if the development of the child should lead directly to the stage reached by the Greeks and to productive thought without going through experimental manipulation, etc., there is always a blocking, which derives partly from the child's feeling of inferiority and partly from the atmosphere, which is not created to favour logical-mathematical comprehension.

This, however, does not explain the first part of what Zazzo said, which was most interesting. In the case of a normal adult (supposing that the student of psychology is a normal adult, which is approximately true, although he nevertheless tends to make mountains out of molehills) the fact that the operations which have been acquired by twelve years are no longer understood later on, does not, to tell you the truth, entirely surprise me. Actually I do not think that this fact is peculiar to problems of conservation; in all kinds of fields, one finds some retardation after schooling and curves which reach their maximum at about fourteen to fifteen years and then decline. I think, then, that we were rather more intelligent at adolescence than later.

ZAZZO:

I am not entirely in agreement. Do you not think that this decline, which is certainly noticeable after schooling, is found particularly among individuals who do not continue to work on an intellectual plane? Among the students I was speaking of there had never been

any discontinuity; they are students all the time; there has never been a break.

PIAGET:

There are those who do not continue in a general sense, and then there are students; but the latter, as specialists, immediately stop thinking in a large number of fields: a student of philosophy, particularly, no longer thinks about physics or mathematics, etc., whereas during school education he is forced to think about the most varied topics, and he is certainly at a higher level for questions which are outside his professional line.

ZAZZO:

But in the example which I borrowed from your work and which implied no special knowledge at all—bearing on the concept of conservation and the water-level in the glass—I don't think that as regards instruction there is any difference between the schoolchild and the student. I think simply that with the student or with the adult there is a sophisticated attitude. This failure proves, in any case, that this operation is very unstable.

PIAGET:

Yes, I agree.

INHELDER:

Your observations correspond to those made by DENNIS (1953), at Brooklyn Teachers' College. A certain number of students of psychology and of education still seem to reason in an animistic fashion. Certainly reasoning is very unstable. This is why I am studying at the moment each kind of reasoning within its functional complex, without isolating it as I used to do when I studied the operational mechanism of the acquisition of concepts of conservation.

LORENZ:

I would like to ask Professor Piaget a question. Are you an empiricist or an a-priori-ist? I have held a grudge of long standing against a colleague of mine in Königsberg who had the Chair some time before me: Immanuel Kant. Mademoiselle Inhelder has used expressions in defining abstractions which are actually derived from Kant, because she said some abstractions are necessary in order to make experience and have to be there before any experience can ever

be made. This is exactly the definition that Kant gives of *a priori*: the 'spectacles' through which we look at things. Space and time, and all the categories which are there before experience is possible, are the conditions under which experience is possible.

In listening to Mlle Inhelder's experiments, I found myself amazed that things which I should have supposed to be *a priori* evidently are not. The same applies to the story which Professor Piaget told us about the number of pebbles. The knowledge that this number will be the same whether we put them in a row or in a heap together, is just one of the things which Kant would have asserted to be quite distinctly *a priori*.

Yet certainly there are some things *a priori*; there must be because we could not begin making experiments without having certain 'working hypotheses' within our central nervous system; for example the working hypotheses that light is projected in a straight line—all our visual reactions are based on that hypothesis. It is a very crude hypothesis, because if light was not straighter in space than it is here on earth where it is deflected by gravity, then one could not see Sirius because its light would go out in curls in space and never arrive at your eyes. A working hypothesis is always a crude basis but is nevertheless there.

Now I think the question of empiricism and *a priori*-ism is not a problem to be solved at all by philosophy, but by research work such as that of Professor Piaget and his school.

PIAGET:

By learning theory.

LORENZ:

Yes, and therefore I think these observations about the pebbles are so extremely important. I should like to believe it is not *a priori*, but I should like some assurance by having a large series of experiments made about these pebbles.

PIAGET:

I should like to thank Dr. Lorenz for his question. My reply will not refer to the sphere of animal psychology, which he knows better than I, but as regards child psychology, I am absolutely convinced that when there is an alternative between empiricism and *a-priorism*. It is always the third way which is correct! In other words, development as we have observed it does not show that either *a-priorism* or empiricism is correct.

Why not *a-priorism*? Because we have never found a ready-made

structure. All structures get constructed, and the example of the order and the cardinal number of pebbles which I quoted the other day is an illustration of this. I even think that in the case of the light in straight lines there is a place for choice and elaboration, because there are several possible geometric schemata: there is the Euclidean straightness, but there could be others too, with curves, and to arrive at Euclidean space, which seems the simplest, and at Euclidean straightness, certainly entails schematization. I think, then, that we shall never find a ready-made piece of knowledge which would be *a priori* or innate. We shall always find it comprises some construction and some experience, or learning as function of experience, but it is here that the distinction comes in between the two types of experience which I tried to introduce the other day.

There are physical experiments, which are experiences in the empiric sense, where we learn little by little, feeling our way, by chance, etc. But there is logico-mathematical experiment, the experiment where the child acts on objects, but where he derives his knowledge not from the object, but from the actions he has applied to objects, and this experimenting leads him to the necessary structures. The necessity which Kant introduced as a criterion of *a priori* is a perfectly exact psychological datum. When he manages to make seriations, and to introduce order into his elements, the child finds the necessary structures; he finds that the sum is necessarily independent of the order; that the order can be reversed; he finds a series of logical relations which appear necessary.

This necessity, however, differs in two ways from Kant's necessity. Firstly, it does not appear as the point of departure but only as the point of arrival for experience. It is a terminal necessity. It is the necessity inherent in a law of equilibrium and not an innate idea which would orient the development from the beginning. It is the necessity corresponding to the most balanced coordination, apart from which there can be no coherent action. The second difference is that it is a necessity which gradually emerges from the distinction between the coordinations of action and the matter on which the subject acts.

I think therefore that the truth lies between the two, although much of what Kant said should be kept in mind: the idea of necessary coordination, but not the *a priori*, because it is a question of a terminal necessity which presupposes an elaboration, a preliminary experience. It is for this reason that I distinguished two types of experience. As soon as one distinguishes them one is no longer an empiricist and one can no longer relate all knowledge to the physical type of experience as the empiricists did. One is faced by a new type of experience which leads simply to necessary coordinations.

Perhaps I have replied in rather an abstract way but you will certainly understand.

LORENZ:

I hope that I understand.

GREY WALTER:

Is it necessary for the human organism to make more than one *a priori* assumption at birth? I should have thought the only *a priori* assumption that a human organism will make at birth is that atmospheric gas inhaled into the lungs supports life.

As to light travelling in straight lines, I should say one's observation of straightness derives from one's observation of light. A straight line is what light travels in.

TANNER:

The question is not what assumption he need make, it is what assumption he does make. This is a matter which can be settled only by experiment, I think.

GREY WALTER:

The new-born infant is not assuming anything, and it rapidly starts to build up a series of hypotheses about what can and what cannot happen. This is opposed to birds and fish which, on the whole, live in rather less variable surroundings and are allowed to make more *a priori* assumptions. We have succeeded uniquely in developing a nervous system which retains a very high degree of plasticity as well as sheer competence.

FREMONT-SMITH:

You think we have freed ourselves from *a priori* assumptions?

GREY WALTER:

Almost. I am not suggesting that *all* the things which are *a priori* or practically innate in other species have to be learnt in ourselves. But we do depend very much on our faculty for investigation and testing.

FREMONT-SMITH:

Do experiments or observations in animals throw light on this? Are there awarenesses in animals which we assume *a priori* which to them are not *a priori*?

Well, I think you have a better opportunity to prove the opposite. Take, for instance, perception of space. Many birds have little or no opportunity of perceiving space when they are sitting in the nest box, because we know that the birds get very little of notion of space by touch. All these very complicated central nervous mechanisms achieving the perception of space are certainly *a priori* in the sense stipulated by the definition. They are something which is existent before experience is made and which must be there in order to make any experience of space possible at all. But they are not *a priori* in the sense of an absolute necessity with which this term is invested in Kant's teaching. I am not quite convinced that any such absolute necessities exist at all. The most stringent necessities, the most universal logical laws, may very possibly be just the consequences of very definite corresponding mechanisms within our central nervous system. These mechanisms make it impossible for us to think in any other ways; hence the 'logical necessity'. And there is no predicting whether such a mechanism is inborn or acquired. In inter-specific comparison we find very often that a function is innate in one species and based on individual learning in another. We certainly must be prepared to find a minimum of inborn and a maximum of learned functions in man. Yet even in man there must be a certain basis of inborn working hypotheses to make the making of experience possible at all. The smaller this basis is, the more interesting it becomes, from the philosophical viewpoint as well as from that of the theory of learning.

REFERENCES

- BATES, J. A. V. (1950). *Electroenceph. clin. Neurophysiol.* **2**, 103.
- BATESON, G. (1949). *Bali: the value system of a steady state*. In: Fortes, M., ed. (1949) *Social Structure*, Oxford.
- BEXTON, W. H., HERON, W., SCOTT, T. H. and HEBB, D. C. (1954). *Canad. J. Psychol.* (in press).
- BOAS, F. (1952). *Primitive art*, New York.
- BOWLBY, J. (1953). *J. ment. Sci.* **99**, 265.
- BOWLBY, J., ROBERTSON, J. and ROSENBLUTH, D. (1952). *A two-year-old goes to hospital*, in: *The psychoanalytic study of the child*, Vol. 7.
- BRADY, J. S. (1954). *Electroenceph. clin. Neurophysiol.* **6**, 473.
- CANNON, W. B. (1936). *Bodily changes in fear, pain and rage*, New York.
- CAROTHERS, I. C. (1953). *The African mind in health and disease: a study in ethnopsychiatry*, Geneva (World Health Organization: Monograph Series, No. 17).
- DENNIS, W. (1953). *Sci. Mon.* **76**, 247.
- ECCLES, J. C. (1953). *The neurophysiological basis of mind*, Oxford.
- EICHLER, R. M. (1951). *J. abn. soc. Psychol.* **46**, 344.
- ERIKSON, E. H. (1950). *Childhood and society*, New York.
- ERIKSON, E. H. (1951). *Amer. J. Orthopsychiat.* **21**, 667.
- EWERT, P. H. (1930). *Genet. Psychol. Monogr.* **7**, 177.
- FARBER, I. E. and SPENCE, K. W. (1953). *J. exp. Psychol.* **45**, 120.
- FORD, C. S. and BEACH, F. A. (1951). *Patterns of sexual behavior*, New York.
- FREUD, A. and BURLINGHAM, D. (1943). *War and children*, New York.
- FREUD, S. (1935). *A general introduction to psychoanalysis*, New York.
- FULTON, J. F. (1943). *Physiology of the nervous system*, Oxford.
- GROSSMAN, C. (1954). Annual meeting Amer. Branch Int. League against Epilepsy, Boston, Aug. 22, 1953.
- HEBB, D. O. (1949). *Organization of behavior*, New York.
- HEBB, D. O. (1953). *Brit. J. anim. Behav.* **1**, 43.
- HEINICKE, C. (1953). *Some antecedents and correlates of guilt-fear in young boys*, Unpublished doctoral dissertation, Harvard University.
- HEINROTH, O. (1911). *Verh. V. intern. ornith. Kongr.* Berlin 1910.
- HERRE, W. (1943). In: Heberer, G. *Evolution der Organismen*, Jena.
- HESS, W. R. (1949). *Das Zwischenhirn*, Basel.
- HICK, W. E. (1950). *Symposium on information theory*, London.
- HYMOVITCH, B. (1952). *J. comp. physiol. Psychol.* **45**, 313.
- INHELDER, B. (1954). *Bull. Psychol.* **7**, 272.
- KOEHLER, O. (1950). In: Society for Experimental Biology. *Physiological mechanisms in animal behavior*, New York.

- LANSDELL, H. C. (1953). *J. comp. physiol. Psychol.* **46**, 461.
- MASSERMAN, J. H. and PECHTEL, C. (1953). *J. nerv. ment. Dis.* **118**, 408.
- MAYR, E. (1942). *Systematics and the origin of species*, New York.
- MEAD, M. (1928). *Coming of age in Samoa*, New York.
- MELZACK, R. (1952). *Canad. J. Psychol.* **6**, 141.
- MELZACK, R. (1954). *J. comp. physiol. Psychol.* **47**, 166.
- MEYER-HOLZÄPFEL, M. (1938). *Z. Tierpsychol.* **1**, 46.
- MIRSKY, I. A., MILLER, R. and STEIN, M. (1953). *Psychosom. Med.* **15**, 574.
- MONNIER, M. (1946, 1948). *Schweiz. Arch. Neurol. Psychiat.* **56**, 233, **325**; **62**, 151.
- MONNIER, M. (1948). *Helv. Physiol. Acta* **6**, 661.
- MONNIER, M. (1952). *J. Neurophysiol.* **15**, 469.
- MONNIER, M. (1953). *Acta neuroveget.* **7**, 84.
- NISSEN, H. W. and RIESEN, A. H. (1949). *Anat. Rec.* **105**, 665.
- PAMPIGLIONE, G. (1953). *Electroenceph. clin. Neurophysiol.* **5**, 622.
- PAVLOV, I. P. (1910). *Work of the digestive glands*, London.
- PAVLOV, I. P. (1927). *Conditioned reflexes*, Oxford.
- PEIPER, A. (1949). *Die Eigenart der kindlichen Hirntätigkeit*, Leipzig.
- PIAGET, J. (1951). *Introduction à l'épistémologie génétique*, Paris.
- PIAGET, J. and INHELDER, B. (1948). *La représentation de l'espace chez l'enfant*, Paris.
- PIAGET, J., INHELDER, B. and SZEMINSKA, A. (1948). *La géométrie spontanée de l'enfant*, Paris.
- PRECHTL, H. F. R. (1949). *Wien Z. Psychol. Pädagog.* **2**.
- PRECHTL, H. F. R. (1953). *Naturwissenschaften*, **40**, 347.
- PRECHTL, H. F. R. and SCHLEIDT, W. M. (1950, 1951). *Z. vergl. Physiol.* **32**, 257; **33**, 53.
- RÉMOND, A. and CONTAMIN, F. (1950). *Rev. neurol.* **82**, 606.
- RÉMOND, A., CONTAMIN, F. and ROUVRAY, R. (1951). *Arch. Sci. phys.* **5**, 19.
- ROBERTSON, J. (1953). *A guide to the film "A two-year-old goes to hospital": a scientific film record*. London.
- SCHAEFER, E. A. (1912). *The endocrine organs*, San Francisco.
- SCHAFFER, H. R. (1954). *Psychol. Rev.* **16**, No. 5.
- SCOTT, J. P., FREDERICSON, E. and FULLER, J. L. (1951). *Personality*, **1**, 162.
- SCOTT, J. P. and FULLER, J. L. (1951). *J. Hered.* **42**, 191.
- SNOW, J. (1855). In: *Snow on cholera*, New York, 1936.
- THOMPSON, W. R. and HERON, W. (1954a). *J. comp. physiol. Psychol.* **47**, 66.
- THOMPSON, W. R. and HERON, W. (1954b). *Canad. J. Psychol.* (in press).
- THORNDIKE, E. L. (1928). *Human Learning*, Ithaca.
- TINBERGEN, N. (1940). *Z. Tierpsychol.* **4**, 1.
- ULLMAN, A. D. (1951). *J. comp. physiol. Psychol.* **44**, 575.

- UTTLEY, A. M. (1954). *Electroenceph. clin. Neurophysiol.* **6**, 479.
WALTER, W. GREY (1953). *The living brain*, London & New York.
WHITING, J. W. M. (1941). *Becoming a Kwoma*, New Haven.
WHITING, J. W. M. and CHILD, I. L. (1953). *Child training and personality*, New Haven.

Index

- Abstraction types, 249–50
Afferent control, 235, 239
Affinity, cerebral, 71–2
Aggressiveness, in restricted animals, 83–4
 innateness of, 91
Alpha rhythms, 67, 70, 247–8
Anencephaly, 239
Animals: *see* Avoidance; Counting; Dogs; Selection
Anxiety, and stress, 106–8
Appetition, to learned behaviour, 133, 135
A-priori-ism, 260–4
Association, learning by, 24, 46
Attachment, capacity for, 87–8
 and mental health, 88–9
Avoidance, in restricted animals, 83
 see also Chimpanzees; Snakes; Strangers
Avoidance responses, stereotyped, in goat, 125–6
- Babinski reflex, 236
 sign of, 236–7
Bates effect, 248
Behaviour, development, Hebb's formulation, 75–6
 homozygosity in, 91
 innate and learnt, 75
 learned, appetite for, 133, 135
 no commonplace, 124–5
 see also Emotional behaviour; Environment; Purposive behaviour
Binary systems, 26
Bindra, Dalbir, biography, 19–20
Blood-letting, 193–4
Buridan's dilemma, 34–5
- Children, development of bonds between, 229
- Chimpanzees, avoidance by, 85–6
Computers, electronic, 26
Concepts, injunctive, 107
Conditioning, Pavlovian, 141 ff
Counting, in animals, 64–5
Crying, control of, 225
- Deprivation, nature of, 139
 sensory, effects of, 96 ff
Development, psychobiological six methods, 21
Displacement, in psychoanalysis, 168
Displacement behaviour, 160 ff
 in young adults, 172
Dogs, effects of hospitalism, 224–5
 experiments on restricted, 76 ff
 inbreeding, 91–2
 see also Isolation; Problem solving; Separation; Superego; Temptation
Domestication, and selection, 95–6
Dreams, child's, 71
- Emotion, functional significance, 128
- Emotional behaviour, development, 83
- Environment, variation in, and behaviour, 75 ff
- Evaluation, 185–6
- Evolution, genetic, 21–2
 reticulate, 96
- Experience, two kinds, 58 ff
 see also Reasoning
- Experiment, logical and physical, 58 ff
- Family, child's imagination of, 200
- Fatigue, 23
- Fears, spontaneous, 84–5
- Frustration, 138, 139

- Genetics, ignorance of human, 92
Gusto, 121
- Hallucinations, 99–106
 personality and, 102
- Homology, 128
- Hospitalism, effects on children, 221–2
 effects on dogs, 224–5
- Hypothalamus, stimulation of, 116 ff, 180
- Hypothyroidism, 130
- Identification, 185–6, 187 ff
- Immunity, to stress, 226
- Imprinting, mammalian analogue, 125
 model, 40 ff
- Inbreeding, results, in dogs, 91–2
- Instinct, 21, 22
- Instinctive movements, development, 174
- Intention movements, 172–4, 181
- Internalization, of rules of conduct, 185
- Interpretation, and learning, 255
- I.R.M., 173–5
- I.R.M.A., 41 ff
- Isolation, of single variables, 25
 social, in experiments with dogs, 77–8
- Kachina, 202–3
- Koehler, Otto, experiments, 64–5
- Kupalov, 141–3
- Learning, effects of restricted opportunity, 82–3
 reinforcement theory, 116 ff
 without tuition, 185 ff
- 'Learning box,' 48
- Liddell, Howard, biography, 17–19
- Locomotion, development of, 79
- Machina speculatrix, 29 ff
- Maternal care, effects of absence of, 77
- Mathematics, aptitude for, 258–9
- Memories, traumatic, 127
- Memory, in IRMA, 49
 types of, 53
- Mescaline, hallucinations, 101–2
- Metabolism, changed, and instincts, 43
- Migraine, hallucinations in, 101
- Models, study of, 25–8
- Monotony, 144–5
- Mother, good and bad, 220–1
 see also Recognition
- Neurosis, in sheep, 127
- Nurturance, 186
- Obsessions, with positive and negative stimuli, 140
- Operator, 28
- Optomotor integration time, 243
- Oral prehensile reflex, 235–6
- Parent-figure, doctor as substitute, 234
- Parents, child's evaluation of respective, 200
- Patient-responsibility, 189–90
- Pattern, and learning, 49
- Pavlov, his procedure, 141 ff
- Plants, psychobiological capacities, 21–2
- Play, and displacement activities, 178–9
- Plenary systems, 27
- Posture, and displacement activities, correlation, 180
- Practice, 21–2
- Problem solving, in restricted dogs, 82
- Punishment, *see* Reward; Self-punishment
- Purposive behaviour, 28
- Reaction-times, 248–9
- Reasoning, and experience, 61
- Recognition of mother, failure in, 231
- Réflexe palmomentonnier, 239
- Reflexive action, 21, 22

- Reindeer, domestication, 95-6
 Reinforcement theory, 116
 Repetition, learning by, 21, 22-4
 Repression, 161-2
 Responsiveness, 108 ff
 Reward and punishment, and learning, 116
 Rossolimo, sign of, 237-8

 Sacrifice, 194
 Sanctions, internal and external, 205
 Selection, deliberate and natural, 93
 natural, in wild animals, 95
 principles of, in humans, 94-5
 Self-punishment, 193 ff
 Separation, avoidable, 226, 227
 critical ages for damage from, 224
 response to, 213 ff
 response of dogs to, 230
 Sin, 190
 Snakes, avoidance, 85-6
 Social communication, 24
 Social organization, 28-9
 Social status, gradations, and child attachment, 233
 Socialization, 186 ff
 Stimulus, conditioned reflex without, 142-3
 specific, differentiation of, 48
 Strain, 218-19

 Strangers, avoidance of, 86-7
 Stress, beneficial factor in, 222, 224
 definition, 181 ff
 differential reactions to, 106 ff
 in learning, 54
 learning under, 123-4
 psychological definition, 217 ff
 see also Anxiety; Immunity
 Sublimation, 138
 Superego, 193 ff
 development in dogs, 208-9
 and patient-responsibility, 205
 Svenster, Piet, 167
 Synapse, contingent unitary, 42

 Temptation, resistance to, in dogs, 207-8
 Thalamus, coagulation of, 105-6
 Thyroidectomy, effects, 131
 Tics, 171
 Time, Newtonian and Bergsonian, 34
 estimation, 144
 Typology, variation in, 146

 Unacceptance, interdoctrinal, 127-8
 Unitary systems, 26, 29

 Weeping, 173

 Yawning, 174



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TAN

DR. JOHN BOWLBY

Director, Children's Department
Tavistock Clinic, London

DR. FRANK FREMONT-SMITH

Chairman
Josiah Macy, Jr. Foundation, New York

DR. G. R. HARGREAVES

Formerly Chief, Mental Health Section
World Health Organization, Geneva
Professor of Psychiatry
University of Leeds

MILLE. BÄRBEL INHELDER

Professeur de Psychologie de l'Enfant
Institut des Sciences de l'Education de
l'Université de Genève

DR. KONRAD Z. LORENZ

Forschungsstelle für Verhaltensphysi-
ologie des Max-Planck-Institutes für
Meeresbiologie
Büldern über Dulmen, West Germany

DR. MARGARET MEAD

Associate Director Dept. of Anthro-
pology
American Museum of Natural History,
New York

DR. K. A. MELIN

Director, Clinic for Col.
Stora Sköndal, Stockholm

DR. MARCEL MONNIER

Chargé de Cours de Neurophysiologie
appliquée, Université de Genève

PROFESSOR JEAN PIAGET

Professeur de Psychologie à la Sorbonne
et à l'Université de Genève

DR. A. RÉMOND

Chargé de Recherches, Centre National
de la Recherche Scientifique, Paris

DR. R. R. STRUTHERS

Formerly Associate Director
Rockefeller Foundation, Paris

DR. J. M. TANNER

Formerly Senior Lecturer,
Sherrington School of Physiology,
St. Thomas's Hospital
Lecturer, Institute of Child Health
University of London

DR. W. GREY WALTER

Director of Research
Burden Neurological Institute, Bristol

RENÉ ZAZZO

Directeur du Laboratoire de Psychobiologie de l'Enfant
Institut des Hautes Études, Paris

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